Two birds with a stone?

The Rural Environmental Registry (CAR) and Its Potential Spillover Effects Over Land Conflicts in the Brazilian Amazon

Nilesh N. Shinde
Ph.D. Candidate,
Department of Forest Resources and Environmental Conservation,
Virginia Tech
Email: nilesh@vt.edu

Stella Z. Schons
Department of Forest Resources and Environmental Conservation,
Virginia Tech

Alexandre Gori Maia
University of Campinas,
Brazil

Gregory S. Amacher
Department of Forest Resources and Environmental Conservation,
Virginia Tech

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Abstract

Insecure property rights emerging from ill-defined laws and burdensome bureaucracy conform to a common institutional picture across the developing world, often leading to a fault line that commences with land speculation and is followed by social conflict over land ownership and deforestation. We assess the effects of a one-of-a-kind environmental management policy established to halt deforestation – the rural environmental registry (CAR for the Portuguese acronym "Cadastro Ambiental Rural") – on conflicts over land in Brazil, a case study of insecure property rights to land conflict. The CAR is mandatory for all private rural properties and does not require a formal title to the land. However, there was anticipation among titleless landholders that registration in the CAR would allow them to declare ownership over the land and a fast track to a formal legal title—the Forest Code of 2012 consolidated previous state-level registration programs of Mato Grosso and Pará. In our analysis, the difference in timing of program implementation in Pará (in 2008) and Mato Grosso (in 2009) in relation to the rest of the Brazilian Amazon (in 2012) allows us to model the CAR as a staggered treatment intervention. Using staggered DiD, Pará (early treatment cohort-2008) has experienced a statistically significant decline in land conflict, with the ATT-by-group showing a 0.547-unit reduction in land conflicts. In contrast, Mato Grosso (early treatment cohort-2009) significantly increases conflicts in the ATT-by-group with 0.471-units. The rest of the states in the Amazon (late treatment cohort-2012) also experienced a significant increase in land conflicts by the ATT-by-group: 0.289-units. Our results suggest that the staggered implementation of the Forest Code through the CAR program had a unique consequence on land conflict across different group-by-year since CAR. In addition, we found a dynamic event study design that increased land conflicts significantly across all groups. We suggest that the long-term implementation of CAR has persistent unintended consequences in terms of increasing land conflicts across the newer development frontier of Amazon. The divergence in conflict events may indicate frequent extensions within 2012 Forest Code registration deadlines since its enactment in 2012, the different status of socio-economic development within Legal Amazon, and the shifting of native forest frontier from states like Pará to Rondônia.

Key words: land conflicts, dynamic event-study, Forest Code 2012, and Brazil

1. Introduction

Insecure property rights emerging from ill-defined laws and burdensome bureaucracy conform to a common institutional picture across the developing world, often leading to a fault line that commences with land speculation and is followed by social conflict over land ownership and deforestation. We assess the effects of a one-of-a-kind environmental management policy established to halt deforestation – the Rural Environmental Registry (CAR for the Portuguese acronym *Cadastro Ambiental Rural*) – on conflicts over land in the Brazilian Amazon, a case study of insecure property rights to land, conflict, and deforestation in the literature. The dilemma around insecure property rights has been extensively discussed within the context of the Brazilian Amazon, given that it has been identified as the primary factor contributing to the deforestation of the most extensive rainforest in the world (Mendelsohn, 1994; Araujo et al., 2009; Alston & Mueller, 2010). Moreover, property rights insecurity has been shown to cause land conflicts (De Oliveira, 2008; Hidalgo et al., 2010; Fetzer & Marden, 2017). In Brazil, the evolution of 'property rights insecurity and land conflicts' is historically founded on coexisting state-led expropriation of non-productive land for agrarian reform and non-state-led appropriation of land from the small landowner. Both have led to inefficient investments in the land as a productive asset and high investments in labor to watch over it (Araujo et al., 2009; Alston & Mueller, 2010).

Brazil has now gone through over 20 years of functional environmental policy and legislation reviews with the ultimate objective of curbing deforestation of the Amazon rainforest. This process has culminated in enacting a recent Forest Code (FC) in 2012, which included the CAR as the primary policy tool (Arima et al., 2014). However, the CAR has also been touted as a "panacea" for effectively solving problems related to tenure security, conflicts, and economic development in Brazil (Reydon et al., 2020); if well implemented, it would create greater cohesion between land management and farmer's livelihoods (Azevedo et al., 2017; Jung et al., 2021). The CAR is mandatory for all private rural properties and does not require a formal title to the land. Landowners are required to register every property with the government through a georeferenced online system, declaring compliance status with the native forest

conservation rules and, when necessary, presenting a plan towards compliance. This way, a CAR consists of a digital document containing information on the ownership, borders of the property, and spatialized information on its environmental condition (Azevedo et al., 2017). The Forest Code defines rules related to the proportion of each land property (according to location, landscape characteristics, and biome) that must be kept as native forest/cover. This proportion is, in general, 80% in the Legal Amazon region. The decision not to require a legal title in the CAR document was meant to include every landowner in the program and allow an opportunity for them to become compliant with the environmental law regardless of formal ownership (and for the government to better monitor deforestation). However, there was an expectation among titleless landholders that registration in the CAR would allow them to declare ownership over the land and a fast track to a formal title.

The Forest Code of 2012 has consolidated previous state-level registration programs of Mato Grosso and Pará. In order to hold properties eligible for rural activity licenses, such as forest plantations for timber, the programs in Mato Grosso and Pará had a similar goal. In addition, these programs aimed to create spatial information systems for officials and landholders regarding the degree of Forest Code compliance on each property (Alix-Garcia et al., 2018). The CAR program in Pará was introduced in 2004 and expanded in 2008, becoming mandatory for all properties. In contrast, Mato Grosso, a similar program called LAU (the Portuguese acronym *Licencia Ambiental Única*), started in 2009 for properties that were compliant with the (then) Forest Code as a prerequisite for the access to specific operational licenses (Alix-Garcia et al., 2018). In our study, the difference in timing of program implementation in Pará (in 2008) and Mato Grosso (in 2009) in relation to the rest of the Brazilian Amazon (in 2012) allows us to model the CAR as a staggered treatment intervention. Our approach to assessing the effects of environmental legislation such as the CAR on land conflict builds upon the conflict and environmental economics literature.

Economic models of land conflict have focused on how landowners behave based on the expected value of their 'contested resources' (Acemoglu & Wolitzky, 2014) and the expected net benefits or rents extracted

from owning the land. These expected net benefits increase with 'monopoly ownership' (Alston et al., 2000) or 'extortion' (De Mesquita et al., 2005), accounting for the costs of change in the capacity of one group to defend or acquire the resources from other groups (Esteban & Ray, 1999; Acemoglu & Wolitzky, 2014). We propose a land conflict analysis framework that includes the CAR as a component in the total effort to obtain effective ownership over the land, ultimately in the form of a land title. We hypothesize that an environmental policy in a format such as the CAR contributes to reducing land conflict, although land conflict reduction is not its primary aim. We test our hypothesis using a staggered difference-in-differences (DID) econometric model using a novel estimation strategy for multiple periods and variations in treatment time (Callaway & Sant'Anna, 2020). We utilize staggered-DID for a municipality-year cross-sectional panel of 5570 municipalities for 19 years from 2001 to 2019. Additionally, we present two-way fixed-effect regression models with ordinary least square (OLS) and Poisson regression to account for our dataset's high number of municipalities with a ‘zero’ value for the number of conflicts.

Our main results reject the CAR program's hypothesis that summarily reduced land conflicts. We find land that conflicts increased in post-intervention periods in Brazil. Using aggregating group-time average treatment effects, we find that Pará has undergone a statistically significant reduction in a land conflict, with the ATT-by-group showing a 0.547-unit reduction in land conflicts. In contrast, Mato Grosso exhibits a significant increase in conflicts in the ATT-by-group with 0.471 units. The rest of the states in the Legal Amazon also underwent a significant increase in land conflicts by the ATT-by-group: 0.289 units. Thus, our results suggest that the staggered implementation of the 2012 Forest Code through the CAR program had unique consequences on land conflict across different group-by-year since CAR. Additionally, we find that the land conflicts increased across all groups using a dynamic event-study design. We find evidence that early treated groups like Pará underwent reduced conflicts while the rest of the Brazilian Amazon states experienced an increase. We suggest that the long-term implementation of CAR has persistent unintended consequences in terms of increasing land conflicts across the newer development frontier of Amazon. The divergence in conflict events may indicate frequent extensions within 2012 Forest Code registration

deadlines since its enactment in 2012, the different status of socio-economic development within Legal Amazon, and the shifting of native forest frontier from states like Pará to Rondônia.

2. Literature review

In this section, we discuss a few critical ideas on conflict studies from an economics perspective and its illustration in the case of Brazil. We discuss three critical economic perspectives on conflict studies: game theory, public, and institutional economics. In the context of Brazil, we review these perspectives to evaluate the primary hypothesis of this study that property claims *via* the 2012 Forest Code may reduce the land conflicts in the Brazilian Amazon.

2.1. Conflict Research in economics

Conflict in economics is a study of impediments to the mutual (peaceful) exchanges, and violence may be seen as a spillover of these exchanges. Kimbrough et al. (2017) summarized the economic approach to studying the conflict as research to explore mutually well-defined (objective) utility functions. These economic models seek to define mutually optimal behavior (i.e., equilibria) such that decision-makers who consciously weigh the marginal cost and benefit of their actions, accounting for the fact that their adversaries are doing the same, will be unable to unilaterally change their strategies to their benefit (Kimbrough et al., 2017; Anderton et al., 2009). Here, in conflict situations, the economic agents shape their choices and preferences based on incentives and information available for their consumption. Thus, economic models implement the value of 'contested resources' (Acemoglu et al., 2012), i.e., the benefit of owning the resources (such as minerals, land, and water), which increases with 'monopoly ownerships' (Alston et al., 2000) or 'extortion' (De Mesquita et al., 2005).

Moreover, the contested resources are accounted for (dynamic) changes in the capacity of one group to defend or acquire the resources of other groups. This may occur with a twofold motive; firstly: to reduce the cost of conflict for the potential attackers (Esteban and Ray, 1999). Secondly, gain the endowment

attached to winning (Acemoglu and Wolitzky, 2014). The conflict occurs as a fundamental problem of balancing production and trade, where the allocation (or rather misallocation) of factors and endowments are affected by conflicting means and modes. The classic example is the 'colonial origin of underdevelopment' (Acemoglu et al., 2001). In this case, appropriation is a means of wealth acquisition coequal with production and trade as a fundamental economic activity.

Anderton et al. (2009) defined the scope of conflict economics in three ways:

Conflict as a choice:
Suppose economics is defined as the study of choices. Consequently, economist sees conflict as a set of choices where the two groups or individuals seek to maximize their net benefit by engaging in 'a strategic contest' (Schelling, 1958). The contest may be violent (such as civil war) or non-violent (increasing rent on land).
Conflicts as exogenous shocks:
An exogenous shock affects the economic outcome. For example, the civil war in Congo affected the country's economic development (Fearon, 2008; Dell, 2010).
Conflict as a form of predations:
A conflict is a form of predation (Skaperdas, 1992) or appropriation (Garfinkel and Skaperdas, 2008).

Besides these three ways, there is a growing interest in inter-group and intra-group economic analysis of conflicts. The empirical evidence incorporates broader categories of conflicts. Sheremeta (2010) and Bellemare (2012) have shown how and why the opportunity cost for winning supersedes the economic cost of conflict. These approaches may be summarized as a behavioral consequence of conflicts where the conflicts induce a preference for cooperation and non-cooperation based on context. Our focus is on the economic approach defined by Skaperdas (1992) and Garfinkel and Skaperdas (2008), where conflict acts as a situation in which agents choose costly inputs, both to themselves and relative to some socially optimal payoff, in pursuit of private payoff framed as wins or loses.

The economic study on conflicts expanded from game theory to (rational) choices and trade-off analysis. We observe the emergence of two strands of conflict studies literature. One was initiated by game theory models (Sheremeta, 2010), and the second was rent-seeking models (Tullock, 1991). Tullock (1991) suggested a workhorse model of conflict: the model illustrated the rent-seeking contests in which contestants exert costly efforts, and their probability of winning is equal to conflict investment. Additionally, North et al. (2009) have proposed a perspective that open access societies strictly limit access to violence while ensuring open access to political and economic activities. The rent-seeking approach has been consolidated by institutional economics. The institutional perspective combines the rent-seeking models with notions of game theory; doing this, numerous researchers have argued how and why 'open access' or 'weakly defined or undefined property' rights may lead to an endogenous conflict process. Here, the focus on the endogeneity of input decision is significant for applied economic analysis (see Acemoglu and Wolitzky (2014); Acemoglu et al. (2012) and Dell (2010)), and there are varying implicit opportunity costs (such as standard gun vs. butter production frontier model). We shall also note that the conflicts can be an efficient outcome (Hirshleifer, 1991; Dixit, 1987). Even So, these outcomes may be violent, however efficient for the dominant group. For example, in a critical study on strategic mass killing, Esteban and Ray (2008) summed up that the initial endowments of one group or country may form an asymmetric contest for fixed resource exhibit. Authors employ the concept of ethnic and income polarization to demonstrate the institutional contrasts in a microeconomic exploration of conflicts.

In summary, the conflicts in economics are seen from the perspectives of weak institutions and policies; moreover, the conflicts can be situated in the literature on the economic efficiency of initial resource allocation and their outcome given the presence of externalities. Subsequently, the conflicts can co-produce an externality, and potential low transaction costs lead to an efficient (yet conflicting) outcome. In the following section, we review the conflicts in the context of Brazil using the critical theoretical perspectives discussed in this section.

2.2. Property rights, land conflicts, and deforestation

In this paper, we consider how the availability of CAR land registration impacts land conflicts. Our empirical model builds on the hypothesis that the CAR is a component in the total effort to obtain effective ownership over the land, ultimately in the form of a land title. We hypothesize that an environmental policy in a format such as the CAR contributes to reducing land conflict, although land conflict reduction is not its primary aim. Although CAR (and Forest Code) do not confer land titles to individuals. The 2012 Forest Code is a re-specification of property rights. It limits landowners' right to clear all their lands, and it confers to society the right to the environmental benefits of protecting native vegetation on each piece of land (Mueller, 2016). Thus far, the Forest Code endorses the argument that property rights are an instrument of society and derive their significance because they help a man form those expectations that he can reasonably hold in his dealings with others (Demsetz, 1974). However, the *de facto* economic understanding of property rights and their impact on land use has expanded in understanding its dynamic and multidimensional nature. The property right is not seen as a one-dimensional 'right' to hold or own land. Instead, property rights are seen as a 'bundle' of rights such as using, possessing, selling, fencing, and excluding the land (Mueller, 2016).

Researchers have argued that the strict implementation of formally specified one-dimensional property rights under the Forest Code may fail to create an incentive structure considering a robust wedge between *de jure* and *de facto* property rights. Here, the *de facto* means that the first person specifies the property rights (an individual claims the land) or the second person (a group assigns rights or norms emerge) while a government determines *de jure* rights with recognized authority (Alston, 2009; Mueller, 2016). This wedge between the *de facto* and *de jure* property rights stem from the uncertainty about whether the *de facto* rights will prevail or whether the *de jure* rights might be invoked by other claimants or by the government. Incentives can arise for unproductive, opportunistic, and defensive behavior (Mueller, 2016).

This disparity between *de jure* and *de facto* property rights manages sub-optimal investments in land productivity and incentivizes land conflicts & environmental degradation (Alston et al., 2009).

Reydon et al.(2018) and Sparovek et al. (2019) reviewed the legal conflicts over an array of land registries and land title claims in Brazil; the authors concluded that although CAR covers more than 80% of the total number of land area under private property registration, its legality is contested as of multiple overlapping, invalid and duplicate polygons in the registry. The disparity in *de facto* vis-à-vis *de jure* property gets exacerbated by contradictions within *de facto* property rights. The inefficiency of property rights stems from weak institutions, initial resource allocation, and externalities. For example, Alston et al.(2009) suggested that the escalating land conflicts in the context of inefficient property rights and relative price incentivize the land invasion, cultivating land conflicts in the frontier economy. Barbier (2019) suggests that the contemporary problem of 'land-use changes, resource exploitation, and conflicts' stems from two structural features of the local economy. Firstly, the spatial location of the population in marginal areas such as the frontier of the Brazilian Amazon[1] implies the migration of the marginal farming population towards frontier land. Secondly, although remote and less favored agricultural lands may be critical outlets for the rural population, it is increasingly commercially oriented economic activities responsible for much of the current expansion of the overall agricultural land base in developing countries (pp.166).

We believe that individual landholders may enroll in CAR, considering CAR is an antecedent to prospective legal land title. Therefore, obtaining CAR is the initial effort in obtaining the legal land title for newly deforested land. Subsequently, CAR may reduce the local land conflicts as the claim over self-declared territory may resolve the dubious claims over land. However, CAR may increase the conflicts in the regions where the CAR registration has significant overlapping claims. Here, the conflicts are actualized as choices

[1] We observe that population density in Legal Amazon increased from 20.31 to 26.53 while in MATOPIBA from 34.80 to 41.97 while Brazil's population density grew from 98.60 to 120.46 during 2001 to 2019.

via initially non-violent rent-seeking for newly deforested land gets converted into violent investments via predation by large landholders or state-led agencies or appropriation by numerous small-scale landholders.

3. Econometric strategy

Our primary econometric strategy exploits the variation in CAR implementation across three cohorts, Pará in 2008, Mato Grosso in 2009, and the rest of the states in Brazil in 2012. Recent literature has underlined the limitations of using two-way fixed effects OLS with municipality-year panel data to estimate difference-in-difference and event-study design (Borusyak et al., 2018; Abraham & Sun, 2018; Goodman-Bacon, 2020; Callaway & Sant'Anna, 2021; De Chaisemartin & d'Haultfoeuille, 2020). Thus, we employ a novel estimation strategy proposed by Sant'Anna & Zhao (2020) with multiple periods and groups, as demonstrated in Callaway & Sant'Anna, (2021).

3.1. Difference-in-difference (DID) and dynamic event study analyses

There are two primary identifying assumptions in our estimation strategy DID; firstly, the treated municipalities (those who registered in CAR with higher proportions of their total area) and control municipalities (those who registered in CAR with a lower proportion of their total area) would have had a similar trajectory of conflicts in pre and post-intervention year. This is a common trend assumption[2]. Secondly, there is 'no anticipation' of treatment in control municipalities. However, the 2012 Forest Code was implemented simultaneously for all Brazilian territories. Therefore, we do not have a 'never-treated' control group to test our hypothesis in the post-2012 period. In order to construct a treated vis-à-vis control municipality, we designed a quasi-experiment strategy using a ratio of the level of CAR registration in all 5570 municipalities at the end of the year 2019. We define a ratio as the proportion of potentially registrable

[2] In this study, we test the common trend assumption in two ways, visually and by using chi square test for hypothesis pre-treatment is equal to zero. The chi-square test suggests that at least one pre-treatment is not equal to zero; for example, using the Legal Amazon sample, only one period in pre-treatment violates the common trend assumption. We find substantial evidence to argue the common trends between the three treatment cohorts in a visual examination. Please refer to Appendix (3).

CAR property area registered under CAR in given municipalities until December 2019[3]. There is a dual purpose behind this strategy: first, to navigate through multiple potentially available registries[4] for a given land parcel and, second, to create a robust measure of municipality-level CAR implementation.

There are fourteen potential land registries for given land parcels to enroll in the Brazilian Amazon. The 2012 Forest Code sought to consolidate these registries, but it is self-declaratory land possession and remains primarily 'waiting for ratification by local government' (Chiavari et al., 2020). Scholars have considered the legal hierarchy among these registries and potential geospatial errors[5]. This study has concluded that although CAR pertains to all Brazilian territories, the primacy of legality of land property registries (i.e., those with legal entitlement) is higher than CAR (as the CAR is self-declaratory and not a legal title). Therefore, to find the precise registration level in a given municipality, one must consider land registered under other existing property registries and deduct it to get potentially available land under CAR. Secondly, we can determine the CAR properties registered per year using our yearly registration data[6]. However, such a measure will overestimate municipalities' share under CAR, primarily as there are significant geospatial overlaps between CAR and other registries such as Terra Legal or INCRA[7]. As illustrated in Figure (1), we create a ratio to measure the area of a given municipality registered under CAR divided by unregistered area and CAR area. Thus, the quantified ratio measures the share of CAR registration with potentially registrable CAR land in a given municipality. Thus, it serves as the variability

[3] We estimated the ratio using municipality-level CAR registration to potential (available) CAR registration using QGIS to divide our sample into two groups: low- registration vs. high- registration municipalities.

[4] Sparovek et al. (2019) and Reydon et al. (2018) listed 15 land registries in Brazil, where CAR, Sistema de Gestão Fundiária – INCRA (Land tenure management system from INCRA)-SIGEF, Terra Legal and Quilombola land are private registries. Please refer Appendix (5)

[5] Reydon et al., (2018) and Sparovek et al., (2019) conducted robust overview of all Brazilian territory to propose an existing legal hierarchy amongst land registries across Brazil.

[6] There are two sources of CAR data, CAR (2020) is large-scale geospatial data source and SICAR (2020) is public records office collections. We merged them to get yearly aggregates of CAR registration per municipality.

[7] As of 2019, we find that indigenous areas and CAR have average 101 sqkm overlap at municipal level. Conservation units and CAR have 81.39 sqkm overlaps and within CAR average 96.3 sqkm overlaps between properties. We measure the within CAR property-level overlaps to control for effect of overlapping claims in CAR registry on land conflicts at municipality level. Please refer Appendix (8).

of CAR intervention at the municipality level. We propose to exploit this variability to evaluate our primary event-study and DID estimation strategy as well as a robustness check strategy.

$$Muncipal\ area = \sum public + private + (urban, water\ and\ road) + unregistered\ and\ undesignated$$

$$\text{Ratio} = \frac{CAR}{(unregistered)+CAR}$$

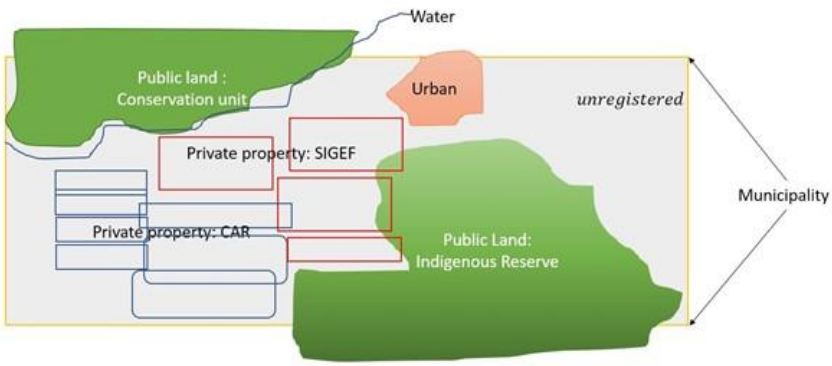

Figure 1 illustrates a representative municipality (grey) as a square outline (yellow) with public land parcels: indigenous reserve and conservation unit (green), urban (orange), private properties: SIGEF (red), and CAR (blue). For illustration, we demonstrate two private land registries in the municipality: CAR and SIGEF. The objective is to measure the area that does not come under public, private, urban, water, and road. We estimated this area at the municipality level. It is assumed to be a potentially available land registered under CAR.

Our primary model specification employs this ratio to divide the sample between low-registration vs. high-registration at the municipality level. As a result, we divide 5570 municipalities into low (1387 municipalities) and high (4183 municipalities) registration. In the primary model specification, we use <25th percentile as a threshold to divide the sample. The foundational assumption is that municipalities with less than the 25th percentile portion of potential CAR area enrolled into CAR (till Dec. 2019) have experienced less treatment intervention intensity. Therefore, the low CAR area registration can be used as a proxy control for the 'low-treated' or control group.

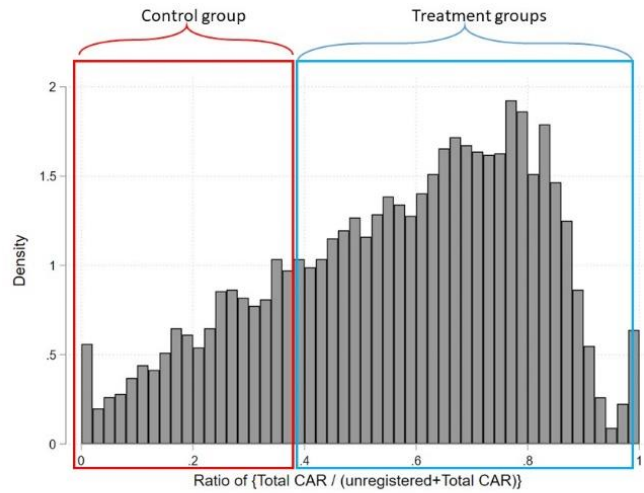

Figure 2 Identification of low vs. high registration municipalities. The figure shows the ratio as a measure of intervention intensity of CAR at a municipal level. The municipalities with no registration in CAR have a ratio equal to zero, and total registration equals one. We divide the sample into low registration municipalities (<25th percentile value) vs. high registration municipalities (>25th percentile value). This allows us to construct a control (low treatment) group for a quasi-experimental setup.

Based on control (low- registration) and treated (high- registration) municipality-level groups, consider a more general case where there are T total periods. Denote periods by t where t=1…, T. By far, the most conventional approach to estimating the effect of a binary treatment in this setup is the two-way fixed effects (TWFE) linear regression:

$$Y_{it} = \theta_t + \eta_i + \alpha D_{it} + \nu_{it}$$

where θ_t is a time fixed effect, η_i is a unit fixed effect, D_{it} is a treatment dummy variable, v_{it} is time-varying unobservable that are mean independent of everything else, and α is presumably the parameter of interest. α is often interpreted as the average effect of participating in the treatment. Although this is essentially a standard approach in applied work, some recent papers present potentially severe drawbacks of using the TWFE OLS estimation procedure. These include Borusyak and Jaravel (2018), Goodman-Bacon (2020), de Chaisemartin and D'Haultfoeuille (2020), Sun and Abraham (2020), and Callaway and Sant'Anna (2021).

TWFE provides unbiased estimates if we have 1) unvarying treatment time, 2) effects that are not heterogeneous, and 3) there are only two time periods. TWFE does not work if we do not validate the above three assumptions. In a TWFE regression, units whose treatment status does not change over time serve as the comparison group for units whose treatment status does change over time. Goodman-Bacon (2018) determines the weights in variance weighted common trends (VWCT) and time-invariant treatment effects using adjustments to group*time interaction dummies in a two-way fixed model. The author argues that the variance weighted ATT (from VWCT) is supposed to be a weighted average of all ATT. However, in the case of differential timing, this weighted average can be erroneous due to biased weights themselves, commonly referred to as a negative weight problem. The problem comes from ‘wrong’ comparisons, i.e., assuming the invariant treatment. If groups receive treatment at different timing, then comparing an already treated cohort with a not-yet-treated cohort is ‘good’ while comparing a late-treated cohort with an already treated cohort is a ‘bad’ comparison. Goodman-Bacon (2018) observed how the “group” variation matters instead of unit-level variation in treatment assignment. If the early treated group is large, they influence the average treated effects more, and vice versa. Goodman-Bacon (2018) corrected this by adding group*time interactions. It is not just a large group but group*time interaction which produces corrected weights to solve the negative weight problem. Additionally, Sant'Anna and Zhao (2020) argued that assuming the invariant treatment and treatment timing, the conical TWFE DID still produces negative weights. This happens because the OLS with group*time interactions puts the same weights on all pre- and post-treatment dummies. The negative weights can emerge from the estimation strategy; therefore, they proposed a new unbiased estimator for two-time and two-group DID. Three practical problems are associated with comparing ‘late treated’ groups to ‘early treated’ groups. We began with group*time interactions; we get 'selection bias' and 'common trend bias' if group-level comparisons are violated. Secondly, we get a 'heterogeneity in time' bias if time-level comparisons are violated. Selection bias occurs if a counterfactual trend and the observed trend are violated, similar to common-trend bias. Thirdly, there can be heterogeneity in time bias. This occurs because implicitly, we have assumed homogeneous treatment, but a 'bad' comparison breaks down that assumption. We can resolve these problems using Sant'Anna and Zhao

(2020), where the conventional TWFE regression-based estimate allows us to obtain consistent treatment effect in case of more than two-period with modifications such as,

$$Y_{it} = \alpha_i + \emptyset_t + \sum_{g=g_0}^{G} \sum_{s=g}^{T} \theta(g,s) * 1(G = g, t = s) + X_{it} + \varepsilon_{it}$$

In Callaway and Sant'Anna (2021), the above specification redefines the group-level ATT as $G \times T$ DID to multiple 2×2 DID (Sant'Anna and Zhao, 2020). Our primary estimator is Sant'Anna and Zhao's (2020) doubly robust DiD estimator based on stabilized inverse probability weighting and ordinary least squares (Sant'Anna and Zhao, 2020). We estimated Callaway and Sant'Anna's (2021) DID using the Rios-Avila et al. (2021) cross-sectional panel with clustered standard errors at the municipality level[8] with multiplicative WildBootstrap procedure.

3.1.1. Identification challenges

Our primary identification challenges deal with two foundational assumptions of DID, firstly the (conditional) parallel-trend assumption (PTA) and secondly, the no anticipation assumption (NA). We propose a primary model that demands an estimation of DID with covariates. Subsequently, the conditional parallel trends are,

$$E[Y_0^0 - Y_0^0 | X, D = 1] = E[Y_1^0 - Y_0^0 | X, D = 0]$$

In other words, the treated group will hold a parallel trend with the untreated group in the absence of treatment intervention. We assume that the municipalities with low or no CAR registration would have a similar conflict trend as high registration municipalities. Note that one caveat is treated, and the untreated group is at a similar level at the beginning. We relax this assumption in order to include the *conditionality*

[8] Rios-Avila et al. (2021) utilizes three approaches for CSDID namely, Sant'Anna and Zhao (2020) Improved doubly robust DiD estimator based on inverse probability of tilting and weighted least squares, Sant'Anna and Zhao (2020) doubly robust DiD estimator based on stabilized inverse probability weighting and ordinary least squares and Abadie (2005) inverse probability weighting DiD estimator. We employ, Sant'Anna and Zhao (2020) doubly robust DiD.

on covariates. Callaway and Sant'Anna (2021) proposed conditional parallel trends (for either never treated or not yet treated) as;

$$E[Y_t^0 - Y_{t-1}^0 | X, Gg = 1] = E[Y_t^0 - Y_{t-1}^0 | X, C = 0]$$

In the above equation, Gg signifies a group and is binary and equals one if individual units are treated at period t. C is also binary and indicates a control group unit equaling one if 'never treated' (in case of not having a 'never-treated' group, then it can be relaxed to 'not yet treated'). In such a setup, the requirement of a 'never-treated' or 'not-yet-treated' group is utilized to measure the ATT.

The no-anticipation assumption states that the treated group does not anticipate the treatment interventions. However, the treated (and untreated) anticipate the 2012 Forest Code intervention in our case. We assume that covariates in our models partially determine the motivation behind the CAR registration (low vs. high). Furthermore, our choice of Callaway and Sant'Anna's (2021) framework also relies on limited treatment anticipation (i.e., treatment effects are zero pre-treatment)[9].

Besides the potential threat of violating the fundamental DID assumption, our primary identification strategy is challenged by dividing the sample into low registration municipalities (< 25th percentile value of ratio) and high registration municipalities. We test using this limitation by conducting our primary DID regression with fully registered municipalities (ratio =1) vs. no registration (=0). Further, we conduct robustness check tests exploiting the variation in CAR registration[10].

There are two challenges in using a ratio threshold as an identification strategy: first, selection bias due to arbitrary ratio threshold, and second, reverse causality of the potential causal channel from low registration

[9] Callaway and Sant'Anna (2021) proposed a chi-square test for 'pre-treatment is zero' under their R and STATA packages. The estimation method is chi-square test to determine the significance of treatment in pre-treatment years or time-periods.

[10] Please refer to Appendix (7); we show the dropping strategy where primary DID regression with fully registered municipalities (ratio =1) vs. no registration (=0) is illustrated.

due to high conflicts. Firstly, a potential selection bias (at the municipal level) via selection in an untreated or treated group due to threshold variations. Note that the registration is universal, i.e., all possible landholders are compulsorily required to self-declare their holdings in the CAR registry[11]. Thus, we assume that the registration in CAR occurred before the end of our cross-sectional panel time represents an aggregated level of CAR registration at the municipality level. The municipalities with less than the 25th percentile threshold have enrolled less than 40% percent of potential CAR land under CAR. Thus, the registration level, in itself, is independent of the period. Furthermore, we find that correlation between CAR registrations and land conflict is weak[12].

Nonetheless, the primary challenge of causative identification with control (low-registration) groups derived with threshold is conditional on the threshold, i.e., ratio. We address this in our robustness check mechanism, where we exploit the variation in CAR registration to get treatment effects across different thresholds. We believe that if the treatment effects are comparable across different thresholds, they hold for the potential threat of selection bias.

[11] The Federal government have repeatedly updated the last enrollment data for CAR. The recent deadline is that the producers must register their rural properties in the CAR no later than December 31, 2020 to be able to join the PRA and benefits from Forest Code's more flexible rules for consolidated areas in Permanent Preservation Area (Área de Preservação Permanente – APP) and Legal Forest Reserve (Chiavari et. al., 2020).

[12] Using yearly municipality-level aggregate of CAR registration area, we find that land conflicts and CAR registration area have 0.17-unit correlation whereas deforestation and CAR registration area have 0.14 unit correlation.

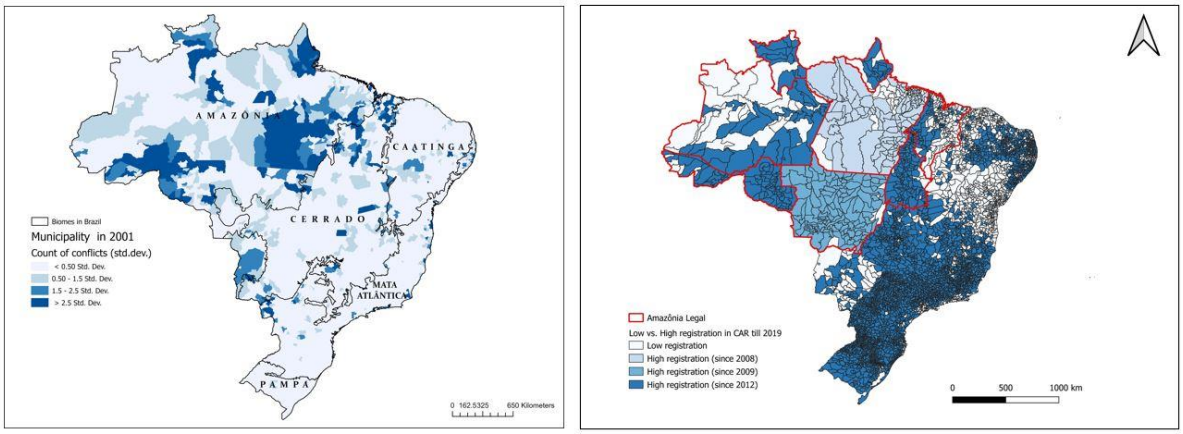

Figure 3 shows that low conflict regions have high registration as well as low registration. We understand that the regions with land conflicts hold on to differences in CAR registration. This can be attributed to different state-level policy implementation strategies—as the CAR is state-subject to implementation and its federal-universal policy. Consequently, we consider the treatment interventions exogenous to land conflicts, i.e., CAR registration at the municipality level.

Secondly, the threat from reverse causality can be addressed by comparing the conflict level in low vs. high registration municipalities. Table (2) observes the comparative summary statistics of municipalities with lower CAR vs. municipalities with higher CAR registration. We observe the disparity in essential variables, viz., the number of conflicts is higher in high registration municipalities. Subsequently, the overlapping area within CAR, indigenous, and conservation units is higher in municipalities with high registration. The annual increment in deforestation is higher in high registration municipalities. Lastly, the correlation between the ratio and the number of land conflicts is weak, 0.061. We consider that treatment intervention—CAR registration can be assumed to be exogenous to our dependent variable—land conflicts.

3.2. Data

The panel data was constructed for spatial-temporal scope for 5570 municipalities from 2001 to 2019. In Table (1), the overview of data collection is presented. This research will draw upon two primary datasets, the Pastoral Land Commission (Comissão Pastoral da Terra—CPT)[13] on land and resource conflicts; CAR

[13] The CPT was founded by the Catholic Church to highlight the plight of landless workers, small farmers, and squatters. Since 1985, the CPT has published an annual report on Conflitos no Campo (Violence in the Countryside).

– Sistema Nacional de Cadastro Ambiental Rural (SiCAR)[14] for spatial data on land registration. We downloaded the yearly books on Conflitos no Campo, i.e., land-related conflicts, from CPT online libraries (CPT, 2020). The files were consistently extracted at the municipality-level conflict event data from 2001 to 2019. Correspondingly, the land registration data–the area of land registered under CAR and overlaps within CAR-was summarized at the municipality level.

The javaScript-based data mining platform of the Google Earth Engine –GEE (Gorelick et al., 2017) extracts, summarizes, and organizes the remotely sensed LULC datasets from MapBiomas collection 5 (Azevedo et al. 2018) on land use and change patterns. The SQL-based data mining platform the Base dos Dados Mais (Carabetta et al., 2020) was employed to extract long-term databases on the socio-economic (IBGE, 2020), crime & mortality (SIM, 2020), economic indicators (PIB, 2020), agriculture crop production data (PAM, 2020) and livestock survey (PPM, 2020). We collected data on environmental fines from the Brazilian Institute of Environment and Renewable Natural Resources and calculated the weighted annual crop and beef price index using Secretaria da Agricultura e Do Abastecimento (SEAB-PR) (Assunção et al. 2015)[15]. The python-based web-scraping tool uses scrapy, reCAPTCHA solver, and NumPy to write a web-scrapper to extract, organize, and save individual properties ids and their respective information Sicar database (SICAR, 2020). We extracted information on more than 350k property ids from Legal Amazon.

The report include data on several measures of land conflict. We focus on measures that appear in the annual reports consistently between 2001 and 2020. The variables are 'disputes', 'murders', 'attempted murders' and 'death threats' (CPT, 2020).

[14] As of December 2019, the total 6,110,418 number of properties with 614,355,082.25 ha area registered in all Brazilian States.

[15] Please see Appendix (2) for agricultural output prices calculation steps

Table 1 Data sources and descriptions

	Variable	Description	Source
		Municipality level land conflict variables	
Y	Dependent	Conflicts = the CPT “disputes” variable	CPT and author's calculation
		Conflict events: the “Escalations” variable, = murders + attempted murders + death threats. Murders attempted: the “Violence” variable, = murders + attempted murders.	
D	Treated	Group variable identifying 'high registration': Ratio of {Total CAR / (unregistered +Total CAR)} (=1 if municipality includes CAR + unregistered land, 0= there is CAR registration)	Author's calculation
T	Time	Year>2012 for D=1, and otherwise 0	Author's calculation
X	Controls	Municipality level other variables	
		ADI: Annual deforestation increment (SqKm)	MapBiomas and author's calculation
		Fine amount: Total Amount in Real of Environmental Fines (adjusted for 2019R$)	IBAMA and MapBiomas
		Non-ag value: Non-agriculture gross value added at current prices is measured by subtracting the Gross value added at current agricultural prices from the Gross added value at total current prices at the municipality level (adjusted for 2019R$)	PIB
		Protected area: Cumulative WDPA protected area (SqKm)	WDPA
		Overlaps in CAR: Cumulative within CAR overlaps (SqKm)	SICAR
		Population density	IBGE and Census years
		Herd density: Cattle herd density (Number of cattle/Municipal area)	SEAB-PR and author's calculation
		Annual index of crop prices	Assunção et al. (2015) approach

NOTE: Data was compiled from various sources for municipal boundaries of Brazil in 2001. We employed an IBGE municipal code for merging and creating cross-sectional panel data. For the annual index of crop prices, we followed the approach stated in Assunção et al. (2015); please refer to cohort-wise summary statistics in Table (2).

3.2.1 Descriptive Statistics

Table (2) shows cohort-wise summary statistics for the Full sample of 5570 municipalities for 19 years. The Low-registration cohort is our control group with 1387 municipalities, while 2008 (101 municipalities), 2009 (135 municipalities), and 2012 (3947 municipalities) are staggered implementation cohorts for the 2012 Forest Code. We refer to Pará (in 2008) and Mato Grosso (2009) as early treated cohorts, while the rest of the states in the Brazilian Amazon (in 2012) as of late treated cohorts.

We observe that number of land conflicts is higher in cohort 2008 (Para), followed by cohort 2009 (Mato Grosso), and lowest in cohort 2012 (rest of the Legal Amazon states). Additionally, the Annual deforestation increment is highest in cohort 2008, followed by cohort 2009, and lowest in 2012. In CAR registration overlaps, cohort 2008 was the highest, followed by cohorts 2009 and 2012.

Cohort 2008 has higher conflicts, deforestation, and an overlapping area under CAR registration. Historically, Pará has been the focal point of land conflicts as it is the frontier of the Amazon biome. Our sample shows that cohort 2008 also has higher local monitoring capacity measured via environmental fine amount and level of indigenous land area. We expect the monitoring efforts to reflect the reduced land conflicts, whereas CAR overlapping area and deforestation shall aggravate the conflict activity. Similarly, in cohort 2009, i.e., Mato Grosso, we observe higher cow-herd density than cohort 2008; this implies higher incentives for cattle-based economic activity. Mato Grosso also exhibits higher land conflicts than inner states in Amazon biome as it is the frontier state. Cohort 2009 has higher deforestation, CAR overlapping area, and conflicts than cohort 2012.

Callaway and Sant'Anna's (2021) approach employ pre-treatment covariates to estimate the group's conditional parallel trend assumption and subsequent ATT. Our summary statistics suggest that the low registration cohort has distinct pre-treatment covariates than early treated cohorts like Pará and Mato Grosso. Nevertheless, the trends are comparable for cohort 2012. Note that staggered implementation

allows us to exploit the treatment assignment variation to estimate unbiased ATT; we understand that cohorts 2008 and 2009 are equivalent while low registration cohorts and cohort 2012 are equivalents. Therefore, we use the "not-yet-treated" option to compare cohorts 2008 vs. 2009 and 2012 vs. low registration cohorts. This allows us to estimate conditionally dynamic DID and group-by-year DID.

Table 2 Cohort wise descriptive statistics for Full Sample

	Low registration Cohort		High registration Cohort 2008-Para		High registration Cohort 2009-Mato Grosso		High registration Cohort 2012	
	N	Mean (SD)	N	Mean (SD)	N	Mean (SD)	N	Mean (SD)
Number of Land conflicts incidences	26353	0.221 (1.091)	1919	0.952 (1.98)	2565	0.346 (0.926)	74993	0.128 (0.797)
Annual Deforestation Increment (Km2)	26353	4.761 (17.042)	1919	63.402 (114.097)	2565	35.963 (65.475)	74993	3.668 (16.827)
Aggregated Indigenous Protected Area (Km2)	26353	288.574 (2979.102)	1919	2593.915 (11098.155)	2565	247.717 (1306.368)	74993	96.878 (1184.368)
Aggregated Overlapping CAR Area (Km2)	26353	6.063 (190.863)	1919	177.312 (676.46)	2565	83.781 (229.537)	74993	45.678 (4590.847)
Price index-Rice	26353	2822.004 (5613.402)	1919	6830.483 (6072.545)	2565	7446.788 (7048.58)	74993	2517.55 (5261.052)
Price index-Corn	26353	4783.145 (3969.224)	1919	5137.552 (3387.13)	2565	4332.139 (4070.406)	74993	7084.43 (4840.689)
Price index-Soy	26353	692.408 (3406.044)	1919	37.899 (146.933)	2565	7985.398 (9719.152)	74993	2744.793 (5567.665)
Price index-Sugarcane	26353	2170.951 (5728.585)	1919	352.842 (1630.463)	2565	1906.89 (5917.599)	74993	3372.985 (7284.548)
Price index-Cassava	26353	6285.505 (9342.197)	1919	9600.639 (9790.233)	2565	1354.504 (3007.579)	74993	1882.657 (4472.507)
Non agriculture gross value added at current prices	23579	1.229e+06 (1.24e+09)	1706	4.153e+08 (1.357e+010)	2289	3.552e+08 (1.272e+09)	67042	3.784e+08 (2.642e+09)
Ratio of cowherd by municipal area in sqkm	25637	23.927 (24.227)	1900	35.51 (33.178)	2527	46.215 (37.436)	74825	48.116 (33.865)
Total Amount in Real of Environmental Fines(2019R$)	26353	586966.48 (9795414.9)	1919	15917228 (85014815)	2565	8650818.7 (28755757)	74993	445029.54 (9971124.4)
Rainfall (mm)	26352	100.166 (52.891)	1900	184.177 (42.43)	2527	145.056 (26.408)	74860	115.983 (38.083)

4. Results

We present results in two sections, dynamic event study and group-by-year DID. Our primary dependent variable is the 'number of land conflicts at the municipality level. The treatment intervention, i.e., the high CAR registration, occurs at a staggered time in three cohorts, Pará (2008), Mato Grosso (2009), and the rest of the states (2012). Control variables include ADI: Annual deforestation increment (SqKm), Herd density: Cattle herd density (Number of cattle/Municipal area), Fine amount: Total Amount in Real of Environmental Fines (adjusted for 2019R$), Non-ag value: Non-agriculture gross value added at current prices is measured by subtracting the Gross value added at current agricultural prices from the Gross added value at total current prices at the municipality level (adjusted for 2019R$), Protected area: Cumulative WDPA protected area (SqKm), Yearly mean precipitation (mm) and Overlaps in CAR: Cumulative within CAR overlaps (SqKm). Agriculture price indices for rice, corn, sugarcane, and cassava are measured using the calculation of agricultural output prices, illustrated by Assunção et al. (2015).

We analyzed three subsamples: firstly, the entire sample with 5570 municipalities with 19 years. It includes low vs. high registration municipalities across the Brazilian territory. Secondly, we analyze the Legal Amazon sample with 808 municipalities with 19 years. Lastly, we analyze the High-overlap sample[16] with 918 municipalities with 19 years.

4.1. Dynamic event study analyses

We present results from a dynamic event study analysis in Figure (4). The figure shows the treatment estimates of the dynamic ATTs (Callaway and Sant'Anna, 2021). ATTs are estimated relative to the period first treated across all cohorts for each period. Our main findings built on event study: dynamic effects suggest that the land conflicts increased in post-intervention periods in all three samples, whereas the post-intervention trend is uncertain in Full sample regressions. In the Legal Amazon sample, conflicts increase

[16] High overlap sample includes municipalities with high overlaps within registered CAR areas.

in six periods and decrease afterward. These results have two policy implications. Firstly, we observe that land conflicts are dynamically evolving across these group cohorts. The results show a diverging trend of increasing and then decreasing land conflicts for early-treated states. This suggests that the time-of-adoption is a vital aspect to reassess in terms of policy implications.

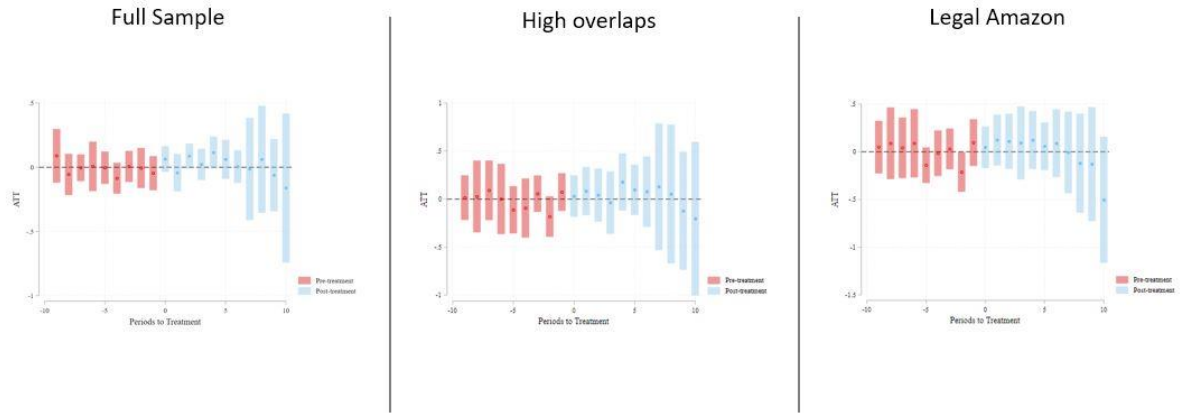

Figure 4 Dynamic event-study design. The results show that the conflicts increased after treatment intervention across three samples. However, there is decreasing increase trend. This suggests that conflicts gradually reduced in the post-intervention period across all three cohorts of early treatment in Pará (2008), Mato Grosso (2009), and the rest of the states (2012). All variables except the dependent variable are log-transformed. The variables are ADI: Annual deforestation increment (SqKm), Herd density: Cattle herd density (Number of cattle/Municipal area), Fine amount: Total Amount in Real of Environmental Fines(adjusted for 2019R$), Non-ag value: Non-agriculture gross value added at current prices is measured by subtracting the Gross value added at current agricultural prices from the Gross added value at total current prices at the municipality level (adjusted for 2019R$), Protected area: Cumulative WDPA protected area (SqKm) and Overlaps in CAR: Cumulative within CAR overlaps(SqKm). All price indices are measured using agricultural output prices calculation steps illustrated by Assunção et al. (2015).

4.2. Difference in Difference with Multiple periods analyses

The dynamic event study suggests that time-of-adoption is a crucial aspect. We expounded on the results of CSDiD implementations proposed by Callaway and Sant'Anna (2020) to infer the cohort-wise results. Using group-by-year DID, we observe that all 2x2 DID estimates (ATTGTs) are estimated using Sant'Anna and Zhao (2020). Our estimation results show the staggered implementation for three group cohorts using a panel data estimator. Note that Callaway and Sant'Anna (2020) suggest the underlying assumption is that

all covariates are time constant. When using panel data, even if covariates are time-varying, only the base-period values are used for the estimation[17].

Table (3) shows results across three samples in our study and three staggered treatment group cohorts. From the perspective of the treated observations, all ATTGTs are estimated using the last 'not-yet-treated' period as the 'base period' and using the current period as the post period. The control groups are selected for the same points in time.

In all samples, we find consistent results that conflict increases in a late-treated cohort 2012 and an early-treated cohort 2009, whereas it decreased in an early-treated cohort 2008. In an early-treated cohort 2008, Pará shows a reduction in conflicts since CAR intervention ranging from 0.54 to 0.36 ATT by a group in Legal Amazon to Full Sample, respectively. We consider that the results for the early-treated cohort 2008-Pará utilizes a 'never-treated' control sample from the late-treated cohort 2012. Similar to the early-treated cohort 2009-Mato Grosso utilizes a 'never-treated' control sample from the late-treated cohort 2012. The results of the two 'early-treated' cohorts count on robust control groups from the late-treated cohort as we utilize the 'not-yet-treated' specification in Callaway and Sant'Anna (2020). We conclude that our results are robust for diverging conflict trends in two early-treated cohorts. However, in the case of the late-treated cohort 2012, we mainly rely on and utilize the quasi-experimental sample of 'low-registration municipalities based on the ratio. Nonetheless, the results are consistent across different samples from the entire Brazilian cross-sectional data.

[17] Additionally, the authors suggest that in using cross section data, while all characteristics can be considered time-varying, the underlying assumption is that within treated and untreated group, characteristics are stationary (time constant). The intuition behind Callaway and Sant'Anna's (2020) estimator is that to obtain consistent estimators for ATT's one should only use never-treated or not-yet-treated units as controls. Otherwise, under heterogeneous treatment effects, the parallel trends assumption will be violated, and the estimations of the effects could be severely biased. (Rios-Avila et. al. 2021).

We consider the results of staggered DID robust under the assumption of quasi-experimental design of low vs. high registration municipalities. However, the value of ATT-by-group varies significantly across the sample for early-treated cohort 2009-Mato Grosso and late-treated cohort 2012-rest of the states due to two key reasons. Firstly, the change in a sample (and the ratio) alters the control group composition. By this means, the estimation of ATT-by-group changes. Secondly, the control municipalities are dependent on the ratio. Our primary model specification holds the ratio constant at low registration (<25th percentile) and adjusts the sample based on the region's characteristics. We find that sign of ATT-by-group stays analogous across the sample. This provides a reliable outcome on the results if the conflicts increased or decreased post-intervention.

Figure (5) illustrates the ATT-group-year results in DID plot. The CSDiD plot illustrates the group-by-year changes in the estimated avg. ATT. The early-treated cohort 2008 observed reduced and persistent conflicts. These results are consistent across different samples. In contrast, the early-treated cohort 2009 observes a consistent increase in conflicts, and these results are not consistent across the different samples. The late-treated cohort 2012 observed a consistent increase in conflicts across the different samples.

Table 3 DID results

	Difference-in-difference with Multiple Periods		
	Full Sample (5570 municipalities & 19 years)	High Overlap Sample (918 municipalities & 19 years)	Legal Amazon Sample (808 municipalities & 19 years)
Avg. ATT			
ATT	0.0341	0.0241	0.0293
	(0.0249)	(0.0807)	(0.0837)
ATT by group			
G2008	-0.3667***	-0.4307**	-0.5468***
	(0.1086)	(0.1573)	(0.1428)
G2009	0.08940	0.1891	0.4713***
	(0.0801)	(0.1319)	(0.1625)
G2012	0.0618**	0.2997**	0.2891**
	(0.0262)	(0.0939)	(0.1089)
H0 All Pre-treatment are equal to 0			
Chi2	35.339	35.414	33.724
p-value	0.01836	0.018	0.0281
Obs.	54376	7235	8264

NOTE: Table shows average treatment effects on treated using Callaway and Sant'Anna's(2021) framework of estimating group-time treatment effect for three group-cohorts Pará(in 2008), Mato Grosso (in 2009), and Rest of the federal states (in 2012). Std. Errors in brackets. Control variables include The variables are ADI: Annual deforestation increment (SqKm), Herd density: Cattle herd density (Number of cattle/Municipal area), Fine amount: Total Amount in Real of Environmental Fines(adjusted for 2019R$), Non-ag value: Non-agriculture gross value added at current prices is measured by subtracting the Gross value added at current agricultural prices from the Gross added value at total current prices at the municipality level (adjusted for 2019R$), Protected area: Cumulative WDPA protected area (SqKm), Yearly mean precipitation (mm) and Overlaps in CAR: Cumulative within CAR overlaps(SqKm). Agriculture price indices for rice, corn, sugarcane, and cassava are measured using the calculation of agricultural output prices, illustrated by Assunção et al. (2015). The estimation was done in Stata CSDiD package using seed number 0687 with 1000 bootstrapping iterations for the "not-yet-treated" specification. All models are with importance weights (iweight) with municipality-level yearly population.

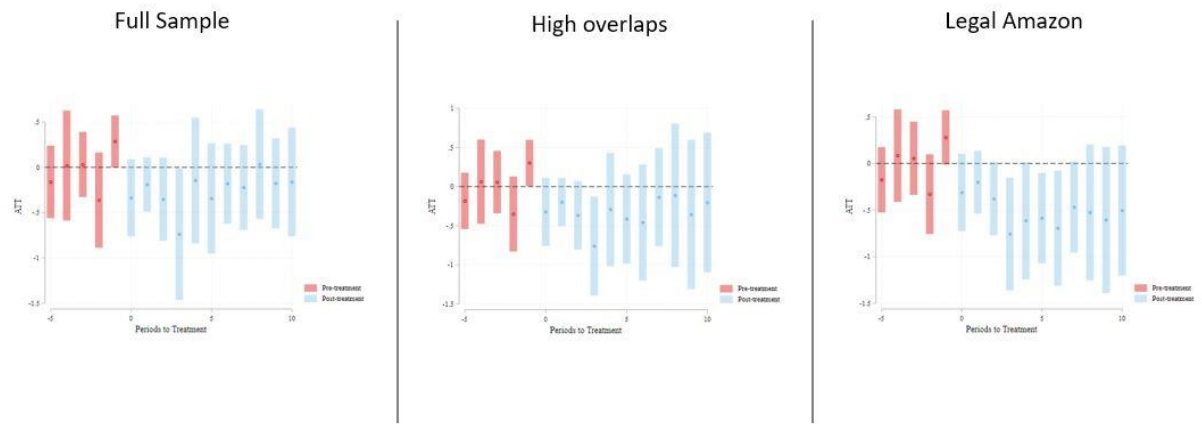

Figure 5a Group cohort: Pará (2008)

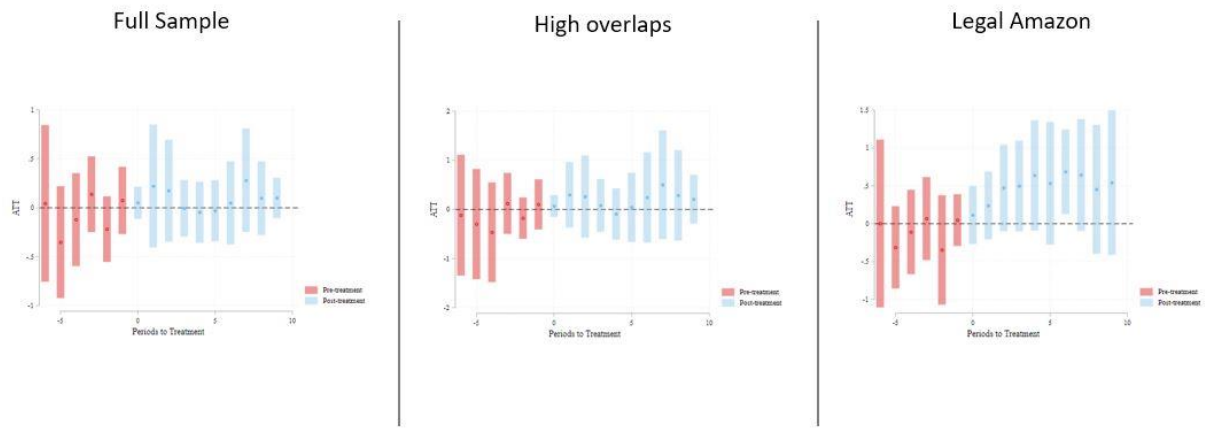

Figure 5b Group cohort: Mato Grosso (2009)

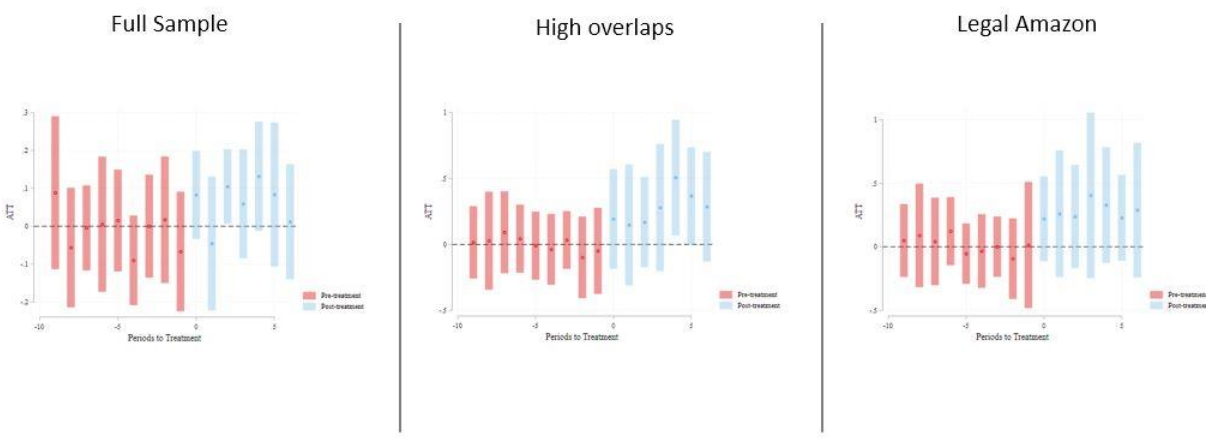

Figure 5c Group cohort: Rest of the states (2008).

Figure 5 shows treatment effects using Difference in Difference with Multiple periods. All variables except the dependent variable are log-transformed. The variables are ADI: Annual deforestation increment (SqKm), Herd density: Cattle herd density (Number of cattle/Municipal area), Fine amount: Total Amount in Real of Environmental Fines(adjusted for 2019R$), Non-ag value: Non-agriculture gross value added at current prices is measured by subtracting the Gross value added at current agricultural prices from the Gross added value at total current prices at the municipality level, Protected area: Cumulative WDPA protected area (SqKm) and Overlaps in CAR: Cumulative within CAR overlaps (SqKm). All price indices are measured using agricultural output prices calculation steps illustrated by Assunção et al. (2015).

4.3. Robustness checks

Our robustness check strategy exploits the variation in CAR registrations across the municipalities. We test our primary model for different combinations of 'low vs. high' registration samples from the entire sample of 5570 municipalities for the same period. We find that our main results hold for the early-treated cohort 2008—Pará and the late-treated cohort 2012-rest of the Legal Amazon states. The early-treated cohort 2009-Mato Grosso exhibits consistency for a sign of ATT-by-group.

We exploit the ratio for robustness checks to address our primary sample's dependence on a ratio to decide on low vs. high registration municipalities. Figure (10) in Appendix (7) shows the sample divided into five low to high CAR registration quantiles. We hold the lowest registration group as a control group and conduct the primary DID model for quintile comparisons. We present results from these comparisons akin to our main model specifications in Appendix (7). Our results remain consistent regarding the ATT-to-group sign, but statistical significance varies across the different samples.

Furthermore, Table (9) shows that our results hold for the 'a number of families in conflicts' from CPT data, 'a number of mortality,' 'a number of mortality men and 'number of mortality women aggregated at municipality-year from the mortality data from SIM. Our results remain consistent with CPT data. Additionally, Table (9) suggests that the conflicts (and mortality) amongst an early-treated cohort of Pará have significantly reduced in post-intervention periods of CAR.

5. Discussion and Conclusion

Our main results suggest that the staggered implementation of the Forest Code in the Brazilian Amazon has unique consequences across different groups depending on their time of adoption of the Forest Code via the CAR registry. The early-treated group Pará (in 2008) has undergone a significant reduction in land conflicts since the adoption of CAR. However, there has been an increase in conflicts in recent periods. In comparison, early-treated groups like Mato Grosso (in 2009) do not exhibit a statistically significant change

in the initial five periods since adoption but show a statistically significant increase in land conflicts afterward. Finally, the late-treated group in the rest of the Brazilian Amazon (in 2012) shows a statistically significant increase in land conflicts since adopting the CAR registry.

These results have two primary policy implications. Firstly, we observe that land conflicts are dynamically evolving across these group cohorts. The results show a diverging trend of decrease and increase in land conflicts for early-treated states, whereas late-treated states show a drastic increase in land conflicts. Therefore, the implementation of the Forest Code needs to be reexamined from state-level policy perspectives. Secondly, our analysis shows a shifting of land conflicts toward the core states like Amazonas of the Brazilian Amazon's native forest. We observe that states like Pará and Mato Grosso exhibit ongoing conflict events, whereas inner states like Amazonas have consistently increasing land conflicts.

These results show a critical insight into the aspect of the regulation of the 2012 Forest Code via analyzing the impact of registration level in CAR on land conflicts. Registration in CAR regulates the owners to maintain 80% of their property under native vegetation; such change in land-use rights changes the production and exchange costs of the owner (Barbier, 2019). For instance, the landowners (newly enrolled under CAR) may try to prey on new unacquired forested land for agriculture to regain their previous output levels, which got compromised under CAR registration. The results suggest that an increase in conflicts increases annual deforestation increment (ADI). This is in corroboration with literature that suggests that conflicts have an adverse impact on resources. In Brazilian Amazon, land conflicts occur due to land grabbing prospects (Loureiro and Pinto, 2005; Alston and Mueller, 2010). Thus, forest loss is positively associated with land conflict incidents.

Additionally, in North and South Brazil, there are different dynamics of deforestation and land conflicts (Sauer, 2018, Franco da Silva and Bampi, 2019). In northern Brazil, we have a peripheral of the Amazon biome. This region holds high invasive and land grabbing activity, contributing to illegal deforestation and

land occupation (Ferrante et al., 2021). The regional variation in land conflicts is a result of variation in deforestation dynamics. Researchers have illustrated that conducting agriculture on forested land involves higher costs, as Barbier (2002) and Barbier and Cox (2004) posited. On the other side, the land enrolled under CAR does not get optimally converted into a forest. In short, the CAR intervention constraints the open-access condition suggested above. Following the dictum, institutional factors, viz. property rights or land conflicts, can shape economic behavior by influencing exchange and production costs (Angelsen 1999). The higher environmental fine is associated with higher land conflicts across Legal Amazon, North, and North-eastern Brazil.

The CAR intervention and frequent land conflicts in the Brazilian Amazon put the increasing cost of clearing land compared to its open access condition. Barbier (2002) and Barbier and Cox (2004) argue that in some regions, the presence of formal and informal institutions may not lead to optimal management of the supply of converted land from the forests. Still, they may have controlled open access exploitation by restricting land clearing and increasing conversion costs. The CAR intervention introduces a similar institutional constraint on open access exploitations in the Brazilian Amazon. Thus, allowing us a testable hypothesis of the effectiveness of policy intervention as an institutional constraint on deforestation. We also show that the land conflicts gradually increased in Brazil's Legal Amazon region in post-intervention periods. The event study dynamic effects shown in Figure (8) suggest that the land conflicts increased in post-intervention periods across three group-level cohorts in Legal Amazon. Figure (8) illustrates that the land conflicts are increasing with decreasing trend (except in the T+7 period). We understand that the resource-based conflicts gradually decrease in the region as resources gradually deplete. Similar trends can be corroborated using Poisson panel data regression methods where the conflicts have a significant positive relationship with the annual increment in forest loss.

In conclusion, we test our hypothesis if CAR intervention has shrunk land conflicts (and related mortality) using a range of robustness checks. We find that the early-treated cohort—Para, has significantly witnessed

a decrease in conflicts and mortality. In contrast, the second early-treated cohort Mato Grosso results in increased conflicts, but they are uncertain in various robustness check strategies. The late-treated cohort – the rest of the Brazilian States witnessed increasing conflicts and mortality like Para. Our findings represent robust causal evidence that CAR has a complex impact on land conflicts while controlling for covariates like annual deforestation, economic activity, and institutional constraints like protected areas.

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Appendices

1: Administrative units in Brazil

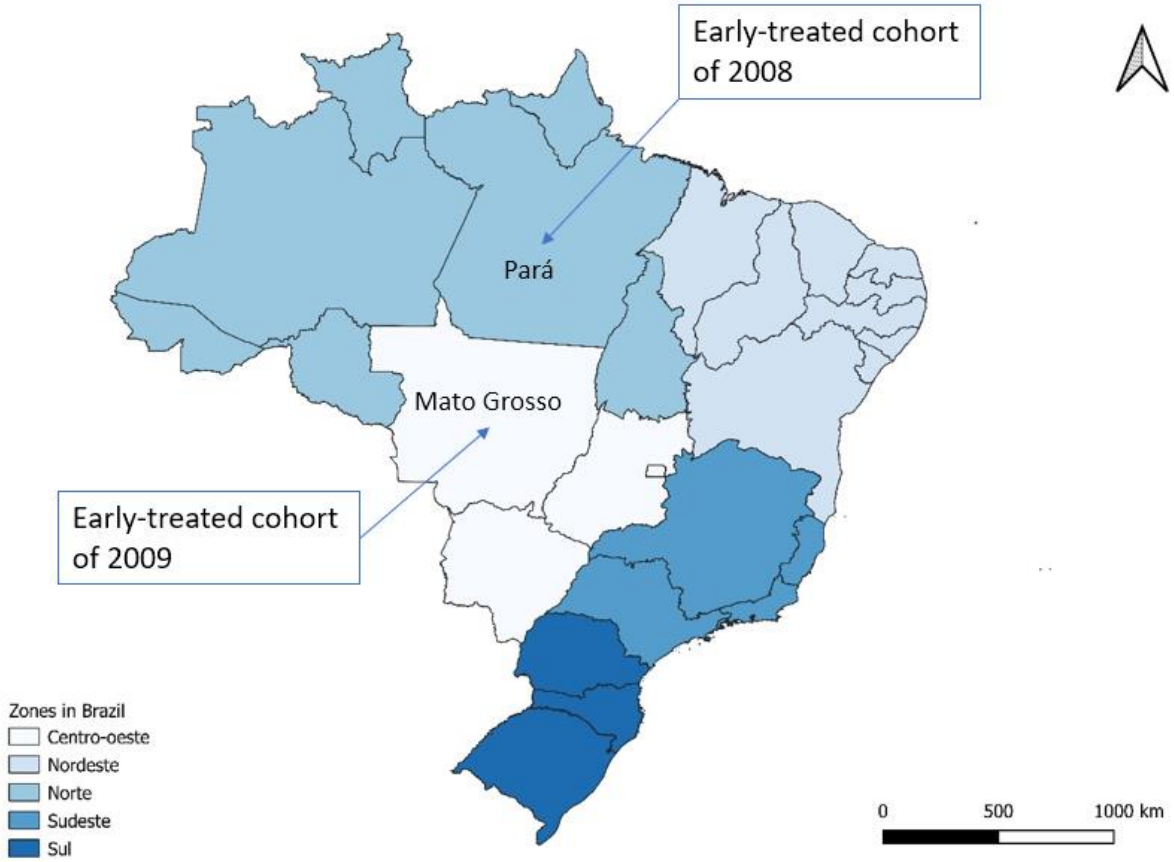

Figure 6 Federal states in Brazil

2: Agricultural output prices calculation steps (Assunção et al. 2015)

We use the Parana price series to build two variables of interest. Parana prices come from, http://www.agricultura.pr.gov.br/deral/precos

1. The first of these variables, an annual index of crop prices, is constructed in three steps.

a. In step one, we construct nominal annual price series by averaging nominal monthly price series for each calendar year and culture. Annual prices are deflated to the year 2000 Brazilian Reais and are expressed as an index with the base year 2000.

b. In step two, we calculate a weighted real price for each of the crops according to the following expression:

$$PPA_{itc} = PP_{tc} * A_{ic}, 2000-2001 \qquad (1)$$

where PPA_{itc} is the weighted real price of crop c in municipality i and year t; PP_{tc} is the Parana-based real price of the crop c in year t expressed as the index with the base year 2000; and A_{ic}, 2000−2001 is the share of the municipal area used as farmland for crop c in municipality i averaged over 2000 through 2001 period. This latter term captures the relative importance of crop c within municipality i's crop production in the years immediately preceding the sample period. It thus serves as a municipality-specific weight that introduces cross-sectional variation in the commodity price series.

c. In the third and final step, we use principal component analysis on the weighted real crop prices to derive the annual index of crop prices. This technique allows the price variations common to the five selected crops to be represented in a single measure. The resulting index of crop prices captures the first principal component of the five weighted real prices. The first principal component explains approximately 38 percent of the variation in the series, driven mainly through soybean, rice, and corn. As the index maximizes the price variance captured by our variable of interest, it represents a more comprehensive measure of the agricultural output price scenario within our empirical setup than the individual prices themselves.

2. The second variable of interest is an annual index of cattle prices, which is derived analogously to PPAitc in equation (1). However, as annual data on land pasture are not available, the index uses the ratio of heads of cattle to municipal area in municipality i averaged over the 2000 through 2001 period as the municipality-specific weight Aci, 2000−2001. Using the annual indices of agricultural prices addresses our model's first empirical implication, which establishes that agricultural output prices should be included in conservation policy evaluation.

3: A note on Common Trend assumption

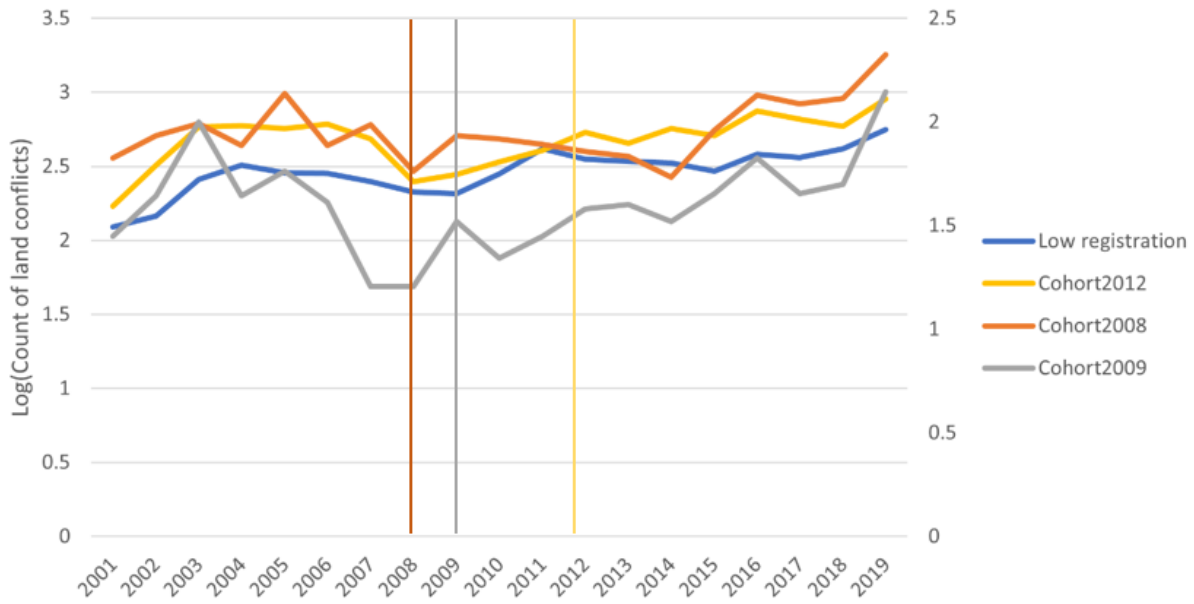

Figure 7 Common trends in land conflict

We conduct a chi-square test with Callway and Sant'Anna's (2021) methodology. The test rejects the conditional common trends assumption. However, we believe that Federal policies like the 2012 Forest Code are enforced simultaneously across states. We reanalyze the assumption with visual representation. The Figure shows that trends across group cohorts are comparable. Further, we conduct a cross-sampled robustness check strategy to verify that trends and signs of ATT across cohorts remain the same for the control vs. treated sample. Our robustness check confirms that ATT measures using primary analysis are replicated throughout the sample.

4: Data from MapBiomas and Land-use changes on different property types

Note on Mapbiomas database:

Our land-use dataset was based on the Collection 5.1 of the MapBiomas Project (Annual Land-Use and Land-Cover Maps of Brazil)1; thus, the secondary forest increment, extension, and age maps are presented here is anchored to the accuracy of the MapBiomas land-use and land-cover dataset. The MapBiomas analyses of accuracy were performed using the Pontius Jr and Millones (2011)[18] method23. For the entire Brazil24, the MapBiomas dataset has an average of $86.40 \pm 0.46\%$ of overall accuracy, $11.06 \pm 0.67\%$ of allocation disagreement, and $2.5 \pm 0.29\%$ of area disagreement between 1985 and 2019, considering the land-use and land-cover classes from the legend level with the most significant detail (level 3). The accuracy assessment for the Brazilian biomes can be found on the MapBiomas accuracy statistics web page (https://mapbiomas.org/en/accuracy-analysis) (Azevedo Sr et al. 2018).

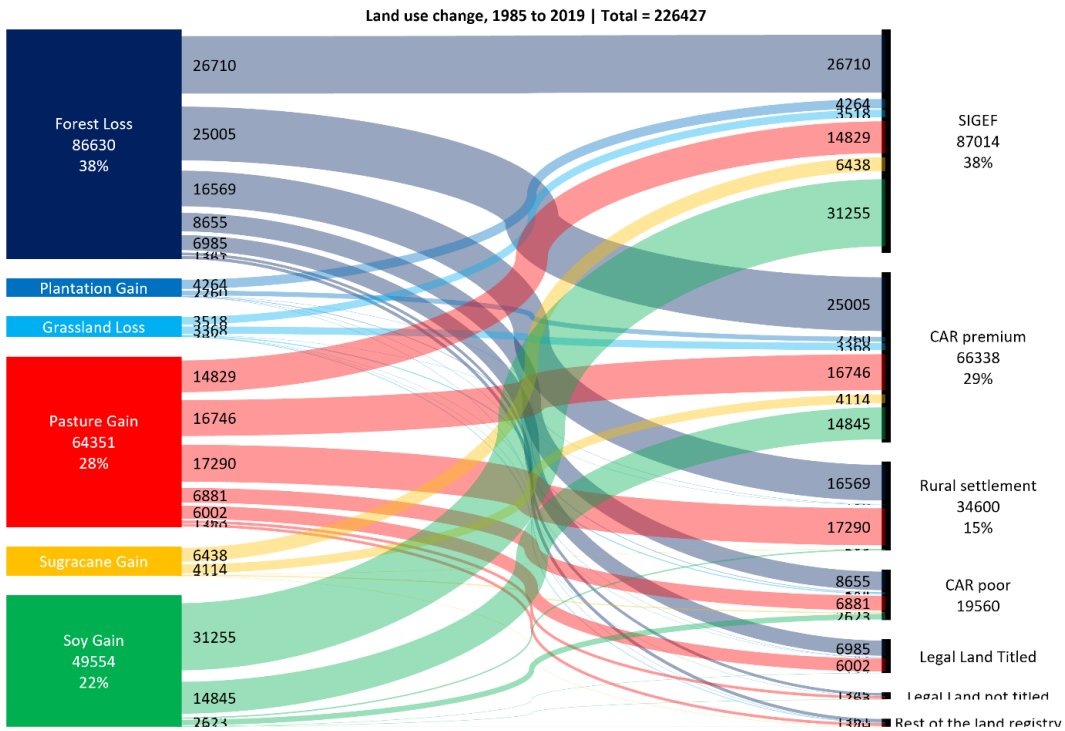

Figure 8 Long term land use changes measured for different land registries using Mapbiomas data in the years 1985-2019. Source: Author's calculation using GEE and Google Colab from Atlas - The geography of Brazilian agriculture, and CAR database

[18] Pontius Jr, R. G., & Millones, M. (2011). Death to Kappa: birth of quantity disagreement and allocation disagreement for accuracy assessment. International Journal of Remote Sensing, 32(15), 4407-4429.

5: Land registries in Brazil till 2018-19

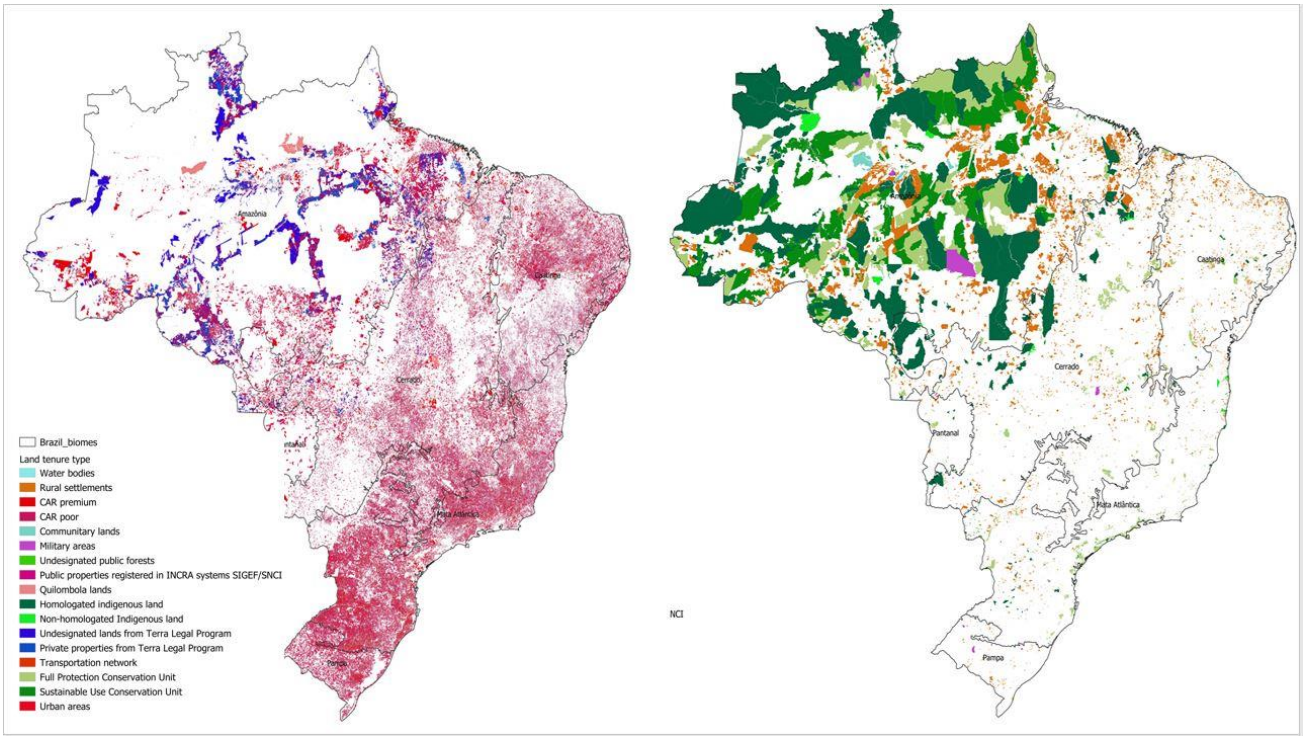

Figure 9 Public and Private properties in Brazil
Source: Author's calculation using GEE and Google Colab from Atlas - The geography of Brazilian agriculture, and CAR database

6: EVENT-STUDY REGRESSION RESULTS

Table 4 dynamic event study using 'number of land conflicts' in Callaway & Sant'Anna (2021)

	(1) Full Sample	(2) Legal Amazon Sample	(3) High Overlap Sample
T-9	0.123	-0.00760	0.0241
	(1.61)	(-0.09)	(0.29)
T-8	-0.120	0.0855	-0.156
	(-1.82)	(0.50)	(-0.81)
T-7	0.00904	-0.00967	0.150
	(0.20)	(-0.06)	(0.86)
T-6	-0.00366	-0.0821	-0.304
	(-0.06)	(-0.44)	(-1.30)
T-5	0.0193	-0.133	0.0391
	(0.35)	(-1.38)	(0.28)
T-4	-0.105*	-0.0627	-0.0909
	(-2.13)	(-0.69)	(-0.89)
T-3	0.000962	0.0166	0.0408
	(0.02)	(0.21)	(0.62)
T-2	-0.00483	-0.199**	-0.182*
	(-0.09)	(-2.69)	(-2.42)
T-1	-0.0477	0.0902	0.0661
	(-1.05)	(1.02)	(0.95)
T+0	0.0634	0.0525	0.0477
	(1.84)	(0.71)	(0.62)
T+1	-0.0426	0.125	0.0958
	(-0.85)	(1.32)	(1.05)
T+2	0.0858*	0.113	0.0464
	(2.54)	(1.10)	(0.49)
T+3	0.0199	0.0860	-0.0352
	(0.47)	(0.63)	(-0.31)
T+4	0.101*	0.0963	0.119
	(2.37)	(0.83)	(1.10)
T+5	0.0530	0.0650	0.0839
	(1.03)	(0.68)	(0.79)

T+6	0.00143	0.115	0.0901
	(0.03)	(0.90)	(0.72)
T+7	-0.102	-0.0440	0.0877
	(-0.69)	(-0.26)	(0.36)
T+8	-0.0710	-0.221	-0.0989
	(-0.57)	(-1.16)	(-0.43)
T+9	-0.0963	-0.173	-0.237
	(-0.94)	(-0.79)	(-1.22)
T+10	-0.297	-0.633**	-0.461
	(-1.38)	(-2.65)	(-1.67)
Observations	54376	7235	8264
	.	.	.

NOTE: The results are from the Outcome model: least squares and Treatment model: inverse probability. All variables except the dependent variable are log-transformed. The variables are ADI: Annual deforestation increment (SqKm), Herd density: Cattle herd density (Number of cattle/Municipal area), Fine amount: Total Amount in Real of Environmental Fines(adjusted for 2019R$), Non-ag value: Non-agriculture gross value added at current prices is measured by subtracting the Gross value added at current agricultural prices from the Gross added value at total current prices at the municipality level, Protected area: Cumulative WDPA protected area (SqKm) and Overlaps in CAR: Cummulative within CAR overlaps(SqKm). All price indices are measured using agricultural output prices calculation steps illustrated by Assunção et al. (2015). t statistics in parentheses and * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

7: Robustness check results

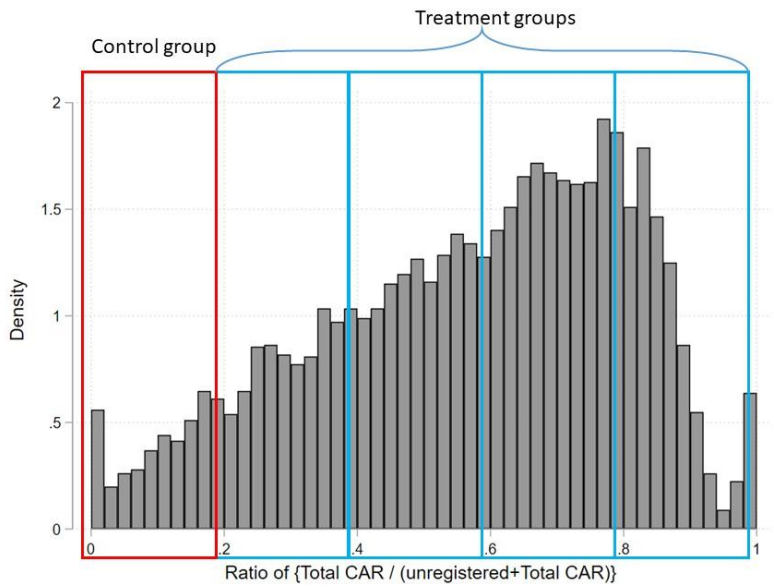

Figure 10 Robustness check identification of low vs. high registration. The figure shows a division of the sample into five quantiles. We conducted our primary DID estimation using a combination of a control group (lowest registration quintile) for the treatment group (from quintile 2 to quintile 4) in a separate estimation model.

Table 5 Robustness Checks: Full Sample

	Quintile 12	Quintile 13	Quintile 14	Quintile 15
ATT	0.0775**	0.1127***	0.0558	-0.0332
	(0.03391)	(0.03398)	(0.0423)	(0.0468)
ATT by group				
G2008	-0.4631	-0.3180	-0.3919*	-0.3661**
	(0.5207)	(0.1959)	(0.2081)	(0.1461)
G2009	0.2529**	0.2639*	-0.1939	0.0123
	(0.1262)	(0.1459)	(0.1941)	(0.0935)
G2012	0.0861***	0.1227***	0.0953**	0.0384
	(0.0313)	(0.3418)	(0.0425)	(0.0529)
Ch2 test	53.545	33.852	30.6714	32.7325
p-value	0.000	0.0272	0.0596	0.0360

NOTE: Table shows average treatment effects on treated using Callaway and Sant'Anna's (2021) framework of estimating group-time treatment effect for three group-cohorts Pará (in 2008), Mato Grosso (in 2009), and Rest of the federal states (in 2012). Control variables include The variables are ADI: Annual deforestation increment (SqKm), Herd density: Cattle herd density (Number of cattle/Municipal area), Fine amount: Total Amount in Real of Environmental Fines(adjusted for 2019R$), Non-ag value: Non-agriculture gross value added at current prices is measured by subtracting the Gross value added at current agricultural prices from the Gross added value at total current prices at the municipality level (adjusted for 2019R$), Protected area: Cumulative WDPA protected area (SqKm) and Overlaps in CAR: Cumulative within CAR overlaps (SqKm). All price indices are measured using agricultural output prices calculation steps illustrated by Assunção et al. (2015). The estimation was done in Stata CSDiD package using seed numbers 0687 and 1000 bootstrapping iterations. All models are with importance weights (iweight) with municipality-level yearly population.

Table 6 Robustness Checks: Legal Amazon Sample

	Quintile 12	Quintile 13	Quintile 14	Quintile 15
ATT	-0.1794	0.0843	-0.1430	-0.1247
	(0.1815)	(0.1315)	(0.1602)	(0.1076)
ATT by group				
G2008	-0.3984	-0.4623*	-0.6305**	-0.4144***
	(0.7541)	(0.2514)	(0.2438)	(0.1426)
G2009	-0.3593***	-0.0483	-0.0893	0.4603**
	(0.1272)	(0.1555)	(0.1773)	(0.1951)
G2012	0.0339	0.4039***	0.2688	0.2051
	(0.1051)	(0.1252)	(0.1963)	(0.1478)
Ch2 test	105.975	37.848	32.324	54.526
p-value	1.7e-14	0.0092	0.0399	0.0000

NOTE: Table shows average treatment effects on treated using Callaway and Sant'Anna's(2021) framework of estimating group-time treatment effect for three group-cohorts Pará(in 2008), Mato Grosso (in 2009), and Rest of the federal states (in 2012). Control variables include The variables are ADI: Annual deforestation increment (SqKm), Herd density: Cattle herd density (Number of cattle/Municipal area), Fine amount: Total Amount in Real of Environmental Fines(adjusted for 2019R$), Non-ag value: Non-agriculture gross value added at current prices is measured by subtracting the Gross value added at current agricultural prices from the Gross added value at total current prices at the municipality level (adjusted for 2019R$), Protected area: Cumulative WDPA protected area (SqKm) and Overlaps in CAR: Cumulative within CAR overlaps(SqKm). All price indices are measured using agricultural output prices calculation steps illustrated by Assunção et al. (2015). The estimation was done in Stata CSDiD package using seed numbers 0687 and 1000 bootstrapping iterations. All models are with importance weights (iweight) with municipality-level yearly population.

Table 7 Robustness Checks: High Overlap Sample

	Quintile 12	Quintile 13	Quintile 14	Quintile 15
ATT	0.0573	0.1792*	-0.0689	-0.1246
	(0.1503)	(0.1017)	(0.1765)	(0.1118)
ATT by group				
G2008	-0.4236	-0.3642	-0.4103	-0.3599**
	(0.5264)	(0.2347)	(0.3365)	(0.1638)
G2009	0.1501	0.3358**	-0.1143	0.2500*
	(0.3512)	(0.1319)	(0.1983)	(0.1310)
G2012	0.1658	0.3393***	0.2758	0.1045
	(0.1121)	(0.1041)	(0.2305)	(0.1877)
Ch2 test	83.8637	23.6275	51.832	51.8622
p-value	1.7e-10	0.2590	0.0001	0.0001

NOTE: Table shows average treatment effects on treated using Callaway and Sant'Anna's(2021) framework of estimating group-time treatment effect for three group-cohorts Pará(in 2008), Mato Grosso (in 2009), and Rest of the federal states (in 2012). Control variables include The variables are ADI: Annual deforestation increment (SqKm), Herd density: Cattle herd density (Number of cattle/Municipal area), Fine amount: Total Amount in Real of Environmental Fines(adjusted for 2019R$), Non-ag value: Non-agriculture gross value added at current prices is measured by subtracting the Gross value added at current agricultural prices from the Gross added value at total current prices at the municipality level (adjusted for 2019R$), Protected area: Cumulative WDPA protected area (SqKm) and Overlaps in CAR: Cumulative within CAR overlaps (SqKm). All price indices are measured using agricultural output prices calculation steps illustrated by Assunção et al. (2015). The estimation was done in Stata CSDiD package using seed numbers 0687 and 1000 bootstrapping iterations. All models are with importance weights (iweight) with municipality-level yearly population.

Table 8 Robustness Checks: Dropping Sample Strategy

	Difference-in-difference with Multiple Periods		
	Full Sample	High Overlap Sample	Legal Amazon Sample
ATT	-0.0229	0.4150**	0.0309
	(0.9560)	(0.1973)	(0.1862)
ATT by group			
G2008	-0.1052	-0.0048	-0.2402
	(0.1969)	(0.2087)	(0.1708)
G2009	-0.1225	0.9323*	0.4409
	(0.1937)	(0.5127)	(0.4305)
G2012	0.0071	0.9249**	0.3590
	(0.1222)	(0.3223)	(0.3181)

NOTE: Table shows average treatment effects on treated using Callaway and Sant'Anna's(2021) framework of estimating group-time treatment effect for three group-cohorts Pará(in 2008), Mato Grosso (in 2009), and Rest of the federal states (in 2012). Control variables include The variables are ADI: Annual deforestation increment (SqKm), Herd density: Cattle herd density (Number of cattle/Municipal area), Fine amount: Total Amount in Real of Environmental Fines(adjusted for 2019R$), Non-ag value: Non-agriculture gross value added at current prices is measured by subtracting the Gross value added at current agricultural prices from the Gross added value at total current prices at the municipality level (adjusted for 2019R$), Protected area: Cumulative WDPA protected area (SqKm) and Overlaps in CAR: Cumulative within CAR overlaps(SqKm). All price indices are measured using agricultural output prices calculation steps illustrated by Assunção et al. (2015). The estimation was done in Stata CSDiD package using seed numbers 0687 and 1000 bootstrapping iterations. All models are with importance weights (iweight) with municipality-level yearly population.

Table 9 Robustness Checks: SIM and CPT data results

	(1)	(2)	(3)	(4)	(5)
	Number of Land Conflicts (CPT)	Number of families in land conflicts (CPT)	Number of Mortality (SIM)	Observed Mortality (SIM)-Women	Observed Mortality (SIM)-Men
ATT	0.0435	0.1183	-0.0652***	-0.0392*	-0.028
	(0.7900)	(0.3378)	(0.0204)	(0.0234)	(0.0171)
G2008	-0.5468***	-2.2119***	-0.0868*	-.0331	-0.183***
	(0.1428)	(0.5167)	(0.2236)	(0.1374)	(0.0595)
G2009	0.4713***	-2.186***	-0.152***	0.2088**	-0.00965
	(0.1625)	(0.6268)	(0.0382)	(0.1043)	(0.0594)
G2012	0.2891***	0.931*	0.0025	-0.0469**	-0.0159
	(0.1089)	(0.3589)	(0.0253)	(0.0236)	(0.0181)
Chi2	33.724	18.385	34.138	19.526	20.338
(p value)	(0.0281)	(0.5621)	(0.0252)	(0.488)	(0.437)

Note: Column (1), (2), and (3) shows results for the Legal Amazon and Column (3), (4), and (5) for the Full sample. Std. error in parentheses and * p<0.05, ** p<0.01, *** p<0.001. The table shows the average treatment effects treated using Callaway and Sant ’Anna’s (2021) framework of estimating group-time treatment effects for three group-cohorts Pará (in 2008), Mato Grosso (in 2009), and the Rest of the federal states (in 2012). Control variables include ADI: Annual deforestation increment (SqKm), Herd density: Cattle herd density (Number of cattle/Municipal area), Fine amount: Total Amount in Real of Environmental Fines (adjusted for 2019R$), Non-ag value: Non-agriculture gross value added at current prices is measured by subtracting the Gross value added at current agricultural prices from the Gross added value at total current prices at the municipality level (adjusted for 2019R$), Protected area: Cumulative WDPA protected area (SqKm) and Overlaps in CAR: Cumulative within CAR overlaps (SqKm). All price indices are measured using agricultural output prices calculation steps illustrated by Assunção et al. (2015). The estimation was done in Stata CSDiD package using seed numbers 0687 and 1000 bootstrapping iterations. All models are with importance weights (iweight) with municipality-level yearly population.

8: Spatial patterns in land conflicts, deforestation, and CAR registration

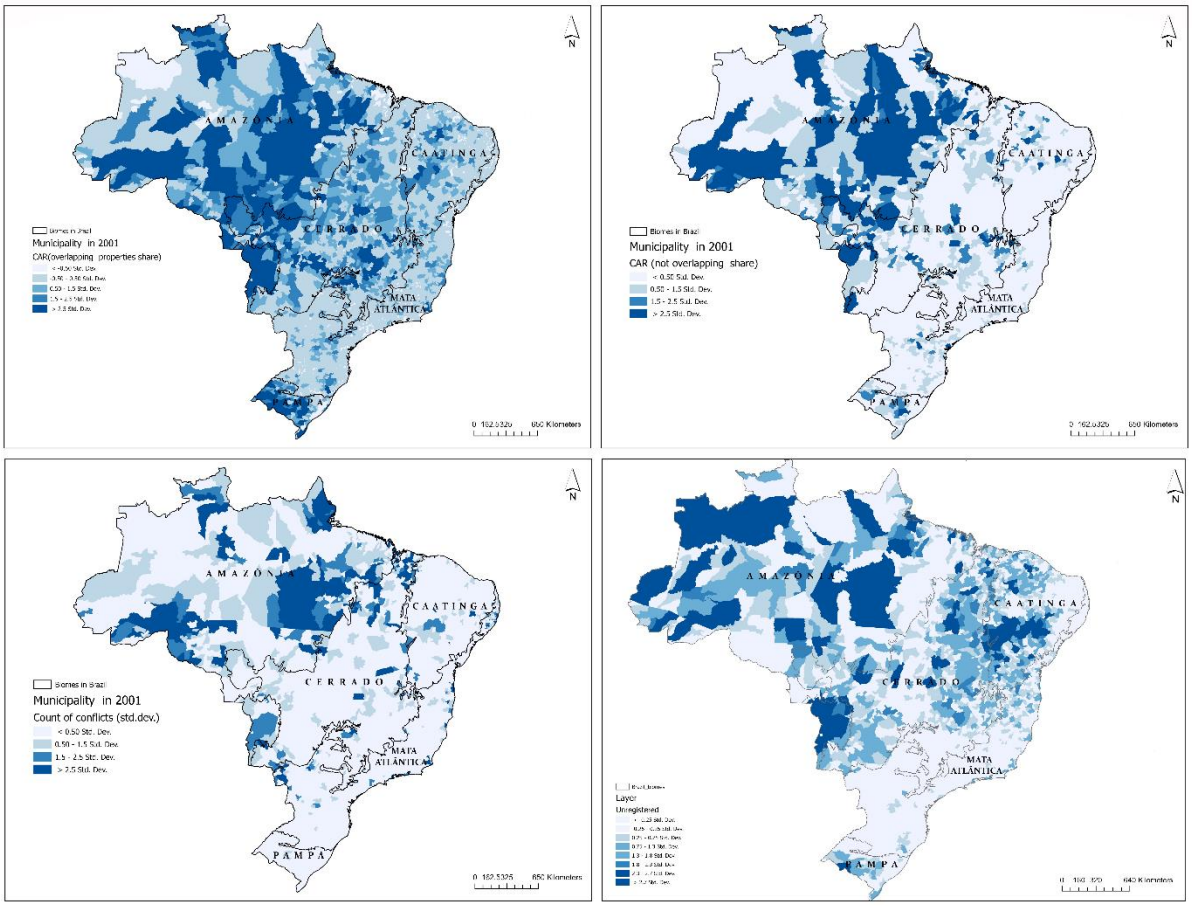

Figure 11
Left panel: Above-overlapping CAR properties; Bottom- count of conflicts
Right panel: Above- not overlapping properties; Bottom -unregistered land.
Source: Author's calculation using CAR, CPT, and Atlas Agropecuario [19] using GEE

[19] Brazil's land network is the result of a collaboration between Imaflora, Esalq / USP's GeoLab, Royal Institute of Technology (KTH-Sweden) and the Federal Institute of Education, Science and Technology of São Paulo. It is the georeferenced database is national in scope, offering an open and public view of the public land and private property in the country. This land network is an update of studies previous studies carried out by the team of Professor Gerd Sparovek - GeoLab of Esalq / USP, in addition to the development of new features and the coding of a routine that allows the permanent update of this database (Freitaset.al. 2017; Freitas et.al. 2018).

9: TWFE OLS and Poisson results

Table 10 Two-way fixed effects linear regression with 'Number of land conflicts'

	(1) Full sample	(2) Legal Amazon	(3) Central-West	(4) Northeast	(5) North	(6) Southeast	(7) South
L0event	-0.0886	-0.0360	0.244*	0.149	-0.0693	-0.329	-0.0144
	(-0.91)	(-0.80)	(1.99)	(1.50)	(-1.02)	(-1.59)	(-0.53)
Annual deforestation increment (SqKm)	0.00850	-0.0135	-0.0185	-0.00570	-0.00388	0.0326**	0.00124
	(0.72)	(-0.85)	(-1.63)	(-0.28)	(-0.22)	(2.62)	(0.09)
Cowherd density (N/SqKm)	0.00679	0.0609	-0.00290	0.0394	-0.0451	-0.0451	0.0255
	(0.25)	(0.79)	(-0.04)	(1.02)	(-0.60)	(-0.82)	(0.77)
Total Amount in Real of Environmental Fines(2019R$)	-0.00112	0.00786	-0.00130	0.00109	0.00228	-0.00429	-0.000279
	(-0.45)	(1.52)	(-0.40)	(0.41)	(0.37)	(-1.23)	(-0.21)
PPA rice	-0.00185	-0.446	0.0818	0.141	-0.606***	-0.0844	-0.0161
	(-0.04)	(-1.93)	(0.66)	(1.77)	(-3.36)	(-1.82)	(-0.21)
PPA_sugarcane	0.0588	0.336*	-0.271***	0.0194	0.367**	0.104	0.102*
	(1.01)	(2.19)	(-3.46)	(0.31)	(3.26)	(0.95)	(2.49)
PPA corn	-0.0743	0.870***	0.0780	-0.0176	0.616**	-0.243	-0.0220
	(-0.54)	(3.95)	(0.62)	(-0.16)	(3.09)	(-1.32)	(-0.35)
PPA cassava	0.148	0.452***	0.00854	0.0482	0.466**	0.135	0.0118
	(1.80)	(4.58)	(0.17)	(1.51)	(3.11)	(1.45)	(0.27)
Precipitation accumulation (mm)	0.0327	0.0373	0.0240	0.136**	0.174	-0.0631	-0.0917
	(0.59)	(0.39)	(0.13)	(2.96)	(1.36)	(-0.62)	(-0.99)
Non agriculture gross value added (R$2019)	-0.0299	-0.0462	-0.121	-0.0282	-0.0328	0.00259	0.0277
	(-0.99)	(-0.46)	(-1.44)	(-0.42)	(-0.26)	(0.09)	(0.90)
Aggregated protected area coverage (SqKm)	0.0198**	0.00927	0.0413**	0.0285*	0.000373	-0.0197	0.0215

	(2.97)	(0.57)	(3.04)	(2.47)	(0.02)	(-1.32)	(1.36)
Aggregated overlap area coverage (SqKm	0.0563**	0.0173	0.00653	0.0180	0.0660	0	0
	(3.11)	(0.58)	(0.25)	(1.19)	(1.86)	(.)	(.)
Constant	0.0141	-6.757**	2.710	-0.789	-4.839	0.985	-0.329
	(0.01)	(-3.23)	(1.42)	(-0.74)	(-1.66)	(0.96)	(-0.32)
Observations	93839	13669	7871	30429	7617	27728	20194
Log-likelihood	-34080.1	-9681.2	-3194.9	-10658.3	-5223.1	-6219.1	2665.3
aic	68222.2	19424.4	6449.8	21370.6	10508.2	12490.1	-5278.5
bic	68515.1	19657.6	6659.0	21595.4	10723.2	12704.1	-5072.8

NOTE: The estimates are using reghdfe with all log-transformed variables where the t statistics in parentheses and * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. All models include municipality-year fixed effects and pre and post-intervention dummies. The std. errors are clustered at the municipality level. All models are with analytical weights (aweight) with municipality-level yearly population.

Table 11 Two-way fixed effects Poisson regression with 'Number of land conflicts'

	(1) Full sample	(2) Legal Amazon	(3) Central-West	(4) Northeast	(5) North	(6) Southeast	(7) South
L0event	0.264**	0.211	0.441	0.570**	0.0768	0.0856	0.452
	(2.63)	(1.79)	(1.03)	(3.14)	(0.46)	(0.25)	(1.63)
Annual deforestation increment (SqKm)	0.0136	0.0522*	-0.0177	0.0164	0.0512	-0.0805	0.163*
	(0.70)	(2.26)	(-0.34)	(0.44)	(1.93)	(-1.46)	(2.17)
Cowherd density (N/SqKm)	0.215*	0.362**	0.100	0.103	0.244	0.157	-0.240
	(2.29)	(3.28)	(0.46)	(0.65)	(1.76)	(0.79)	(-0.73)
Total Amount in Real of Environmental Fines(2019R$)	0.0129**	0.0135*	0.0319**	-0.00101	0.0141	0.0199	-0.00753
	(2.82)	(2.53)	(3.25)	(-0.16)	(1.92)	(1.91)	(-0.44)
PPA rice	0.394*	-0.384	0.139	0.709**	-0.838*	-0.177	-0.875
	(2.57)	(-1.14)	(0.21)	(3.28)	(-2.34)	(-0.54)	(-0.95)
PPA_sugarcane	0.150	0.322	-1.037***	0.0728	0.711***	0.528	-0.0118
	(1.21)	(1.88)	(-3.74)	(0.35)	(3.41)	(1.78)	(-0.04)
PPA corn	-0.0170	1.784	0	0.142	1.363	-0.0148	0.780
	(-0.04)	(1.73)	(.)	(0.34)	(1.51)	(-0.01)	(0.25)

PPA cassava	0.507***	1.015*	0.478	0.766***	1.253***	0.0285	0.726*
	(4.83)	(2.54)	(1.60)	(3.97)	(3.84)	(0.16)	(2.24)
Precipitation accumulation (mm)	-0.0318	0.182	-0.0807	0.499***	0.0764	-0.894***	0.572
	(-0.35)	(1.01)	(-0.25)	(3.50)	(0.32)	(-4.24)	(0.97)
Non agriculture gross value added (R$2019)	0.0364	-0.305	-0.409	0.164	-0.402	0.589**	-0.145
	(0.31)	(-1.67)	(-1.83)	(0.93)	(-1.66)	(2.89)	(-0.66)
Aggregated protected area coverage (SqKm)	0.0642**	-0.00570	0.150**	0.102**	-0.0157	0.0359	0.194*
	(2.99)	(-0.23)	(2.98)	(3.27)	(-0.70)	(0.64)	(2.02)
Aggregated overlap area coverage (SqKm)	0.0625*	-0.0316	-0.0504	-0.0187	-0.00831	0	0
	(2.36)	(-0.94)	(-0.90)	(-0.52)	(-0.18)	(.)	(.)
Constant	-10.96**	-18.59*	3.633	-20.36***	-11.41	-15.35	-8.949
	(-2.86)	(-1.99)	(0.50)	(-4.11)	(-1.48)	(-1.79)	(-0.33)
Observations	34822	9794	4386	13526	5391	6674	4845
Chi2	177.4	151.4	135.5	131.4	714.0	124.7	158.2
Log-likelihood	-25305.5	-10195.0	-2839.0	-10068.0	-5761.7	-3719.7	-2189.1
aic	50673.1	20452.1	5736.0	20190.1	11585.3	7491.4	4430.2
bic	50935.3	20675.0	5921.2	20392.9	11789.7	7668.3	4598.9

NOTE: The estimates are using ppmlhdfe with all count-dependent variables where the t statistics in parentheses and * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. All models include municipality-year fixed effects and pre and post-intervention dummies. The std. errors are clustered at the municipality level. All models are with analytical weights (aweight) with municipality-level yearly population.

Table 12 Two-way fixed effects Poisson-IRR regression with 'Number of land conflicts'

	(1) Full sample	(2) Legal Amazon	(3) Central-West	(4) Northeast	(5) North	(6) Southeast	(7) South
L0event	1.303**	1.234	1.554	1.769**	1.080	1.089	1.571
	(2.63)	(1.79)	(1.03)	(3.14)	(0.46)	(0.25)	(1.63)
Annual deforestation increment (SqKm)	1.014	1.054*	0.982	1.017	1.053	0.923	1.177*
	(0.70)	(2.26)	(-0.34)	(0.44)	(1.93)	(-1.46)	(2.17)
Cowherd density (N/SqKm)	1.240*	1.437**	1.106	1.109	1.276	1.170	0.787
	(2.29)	(3.28)	(0.46)	(0.65)	(1.76)	(0.79)	(-0.73)
Total Amount in Real of Environmental Fines(2019R$)	1.013**	1.014*	1.032**	0.999	1.014	1.020	0.992

	(2.82)	(2.53)	(3.25)	(-0.16)	(1.92)	(1.91)	(-0.44)
PPA rice	1.483*	0.681	1.150	2.032**	0.432*	0.838	0.417
	(2.57)	(-1.14)	(0.21)	(3.28)	(-2.34)	(-0.54)	(-0.95)
PPA_sugarcane	1.162	1.380	0.354***	1.075	2.037***	1.696	0.988
	(1.21)	(1.88)	(-3.74)	(0.35)	(3.41)	(1.78)	(-0.04)
PPA corn	0.983	5.955	1	1.152	3.910	0.985	2.182
	(-0.04)	(1.73)	(.)	(0.34)	(1.51)	(-0.01)	(0.25)
PPA cassava	1.660***	2.759*	1.613	2.152***	3.502***	1.029	2.068*
	(4.83)	(2.54)	(1.60)	(3.97)	(3.84)	(0.16)	(2.24)
Precipitation accumulation (mm)	0.969	1.199	0.922	1.647***	1.079	0.409***	1.772
	(-0.35)	(1.01)	(-0.25)	(3.50)	(0.32)	(-4.24)	(0.97)
Non agriculture gross value added (R$2019)	1.037	0.737	0.664	1.178	0.669	1.801**	0.865
	(0.31)	(-1.67)	(-1.83)	(0.93)	(-1.66)	(2.89)	(-0.66)
Aggregated protected area coverage (SqKm)	1.066**	0.994	1.162**	1.108**	0.984	1.037	1.214*
	(2.99)	(-0.23)	(2.98)	(3.27)	(-0.70)	(0.64)	(2.02)
Aggregated overlap area coverage (SqKm)	1.064*	0.969	0.951	0.982	0.992	1	1
	(2.36)	(-0.94)	(-0.90)	(-0.52)	(-0.18)	(.)	(.)
Observations	34822	9794	4386	13526	5391	6674	4845
Chi2	177.4	151.4	135.5	131.4	714.0	124.7	158.2
Log-likelihood	-25305.5	-10195.0	-2839.0	-10068.0	-5761.7	-3719.7	-2189.1
aic	50673.1	20452.1	5736.0	20190.1	11585.3	7491.4	4430.2
bic	50935.3	20675.0	5921.2	20392.9	11789.7	7668.3	4598.9

NOTE: The estimates are using ppmlhdfe-irr with all count-dependent variables where the t statistics in parentheses and * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. All models include municipality-year fixed effects and pre and post-intervention dummies. The std. errors are clustered at the municipality level. All models are with analytical weights (aweight) with municipality-level yearly population.

10: Summary statistics for Legal Amazon

Table 13 Summary statistics for Legal Amazon Sample

	Low registration cohort		High registration cohort of 2008-Para		High registration cohort of 2009-Mato Grosso		High registration cohort of 2012	
	N	Mean (SD)	N	Mean (SD)	N	Mean (SD)	N	Mean (SD)
Number of Land conflicts incidences	4408	0.596 (2.084)	1919	0.952 (1.98)	2565	0.346 (0.926)	6460	0.7 (2.202)
Annual Deforestation Increment (Km2)	4408	18.213 (37.110)	1919	63.402 (114.097)	2565	35.963 (65.475)	6460	24.263 (51.505)
Aggregated Indigenous Protected Area (Km2)	4408	1544.731 (7112.389)	1919	2593.915 (11098.155	2565	247.717 (1306.368)	6460	999.488 (3904.199)
Aggregated Overlapping CAR Area (Km2)	4408	35.373 (465.438)	1919	177.312 (676.46)	2565	83.781 (229.537)	6460	529.841 (15634.712)
Price index-Rice	4408	7639.054 (7069.514)	1919	6830.483 (6072.545)	2565	7446.788 (7048.58)	6460	10209.981 (6711.983)
Price index-Corn	4408	3905.362 (2933.1)	1919	5137.552 (3387.13)	2565	4332.139 (4070.406)	6460	5353.77 (3262.014)
Price index-Soy	4408	920.866 (4178.014)	1919	37.899 (146.933)	2565	7985.398 (9719.152)	6460	1233.571 (4252.876)
Price index-Sugarcane	4408	209.706 (812.228)	1919	352.842 (1630.463)	2565	1906.89 (5917.599)	6460	411.339 (2226.476)
Price index-Cassava	4408	10757.491 (11832.343)	1919	9600.639 (9790.233)	2565	1354.504 (3007.579)	6460	5496.099 (8231.746)
Non agriculture gross value added at current prices	3944	4.329e+08 (3163383578.796)	1706	4.153e+08 (1357357269.046)	2289	3.552e+08 (1271556433.337)	5780	2.084e+08 (789327860.105)
Ratio of cowherd by municipal area in sqkm	4389	14.837 (20.61)	1900	35.51 (33.178)	2527	46.215 (37.436)	6460	41.966 (42.263)
Total Amount in Real of Environmental Fines(2019 R$)	4408	1834200.1 (18749778.526)	1919	15917228 (85014815.42)	2565	8650818.7 (28755756.54)	6460	3404675.6 (31325427.953)
Rainfall (mm)	4408	164.235 (61.516)	1900	184.177 (42.43)	2527	145.056 (26.408)	6460	149.76 (39.716)

11: Dynamic event study-alternative procedure

Table 14 Dynamic Event Study using Borusyak et al. (2018) with 'Number of land conflicts'

	(1) Without covariates	(2) With covariates
tau0	0.981**	0.977**
	(-2.60)	(-3.02)
tau1	0.970***	0.969***
	(-4.07)	(-4.18)
tau2	1.001	1.001
	(0.12)	(0.12)
tau3	0.987	0.983*
	(-1.73)	(-2.13)
tau4	0.983*	0.979*
	(-1.99)	(-2.25)
tau5	0.986	0.983
	(-1.68)	(-1.80)
tau6	0.970***	0.971**
	(-3.33)	(-2.95)
tau7	0.964***	0.958
	(-3.43)	(-0.96)
tau8	1.037	0.956
	(1.10)	(-0.98)
tau9	1.000	0.922
	(-0.01)	(-1.78)
tau10	1.193***	1.039
	(4.29)	(0.54)
tau11	1.321***	
	(4.17)	
pre1	0.959***	0.960***
	(-4.04)	(-3.60)
pre2	0.966***	0.966***
	(-3.76)	(-3.39)
pre3	1.001	1.004
	(0.12)	(0.39)

pre4	0.989 (-1.29)	0.991 (-0.96)
pre5	1.000 (0.00)	1.003 (0.38)
pre6	1.026** (2.81)	1.032** (3.29)
pre7	0.983* (-1.97)	0.992 (-0.88)
Annual deforestation increment (SqKm)		1.001 (0.46)
Cowherd density (N/SqKm)		1.004 (0.52)
Total Amount in Real of Environmental Fines (2019R$)		1.001 (1.91)
PPA rice		1.029** (2.69)
PPA_sugarcane		1.010 (0.81)
PPA corn		1.040 (0.97)
PPA cassava		1.016** (3.21)
Precipitation accumulation (mm)		1.013 (1.81)
Non-agriculture gross value added (R$2019)		1.025* (2.53)
Aggregated protected area coverage (SqKm)		1.011*** (3.67)
Aggregated overlap area coverage (SqKm)		1.012 (1.37)
Observations	105830	93839

Exponentiated coefficients; *t* statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$